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THE RÔLE OF INORGANIC AGENCIES IN THE DEPOSITION OF CALCIUM CARBONATE

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Organic agencies are doubtless the predominant occasion of the deposition of calcium carbonate, yet certain inorganic factors may not safely be left out of account. The mode of action of the former, which is in part a biological question, we shall not enter into, but we shall limit ourselves to a discussion of the effects producible by variation of certain inorganic factors which affect directly the solubility of calcium carbonate. The question of the concentration of calcium relative to the limiting saturation concentration of calcium carbonate under the particular conditions—in other words, the relative degree of saturation with respect to calcium carbonate—has not received adequate consideration; this is largely the consequence of faulty data and of contradictory and erroneous statements regarding the solubility of calcium carbonate under various conditions. It is our purpose to direct attention to the quantitative effect, as deduced from laboratory study, producible by variation of those factors which, by affecting directly the degree of solubility of calcium carbonate, induce its precipitation from a solution saturated with it; and to emphasize the fact that many of the points now ambiguous may be settled by means of systematic and accurate investigation

of a certain group of properties of sea-water, properties which, moreover, are of high importance in connection with certain biological problems. The mode of treatment is similar to that employed by Stieglitz,[1] who, at the instance of Chamberlin, carried out series of calculations to ascertain the proportion of $CaCO_3$ which one might expect to find in gypsum that had been deposited from solutions saturated with respect to both $CaSO_4$ and $CaCO_3$ at different partial pressures of CO_2 in the atmosphere in contact with the solution. The principles, therefore, are not new, though the point of view differs somewhat; and we now have the advantage of more extensive data than were available in 1907.

The data bearing on the solubility of pure calcite have been collated and discussed at length in two recent papers,[2] to which the reader desirous of fuller information on the chemical side is referred. As it would lead too far to discuss here all details of the solubility-product constant and of its mode of calculation, we shall give only the established conclusions which are pertinent to the present discussion, premising that a symbol inclosed within brackets represents the concentration (expressed in moles per liter) of that particular ionic or molecular species.

1. In a solution at a fixed temperature saturated with pure calcite, the solubility-product—i.e. $[Ca^{++}]\,[CO_3^{=}]$, the product of the respective concentrations of calcium-ion and carbonate-ion—is a constant, independent of the proportion of free CO_2 in the solution and of the presence of other salts. This characteristic solubility-product constant is to be carefully distinguished from the solubility which, as ordinarily measured, is the concentration of *total* calcium in a solution in equilibrium with solid calcite; and this calcium is associated with bicarbonate and hydroxide (and with any other anion present, e.g., chloride or sulphate) as well as with carbonate—indeed, under ordinary atmospheric conditions but a small fraction of the total

[1] J. Stieglitz, "The Relations of Equilibrium between the Carbon Dioxide of the Atmosphere and the Calcium Sulphate, Calcium Carbonate, and Calcium Bicarbonate in Water Solutions in Contact with It," in "The Tidal and Other Problems," by T. C. Chamberlin *et al.*, *Carnegie Inst. Publ. No. 107* (1909).

[2] J. Johnston, *Jour. Am. Chem. Soc.*, XXXVII (1915), 2001, hereinafter designated for convenience as *op. cit.*; Johnston and Williamson, *ibid.*, XXXVIII (1916), 975.

calcium is ever associated with carbonate. The fact of the constancy of this solubility-product in presence of solid calcite[1] enables us to calculate, with all the accuracy required for the purposes of this paper, the solubility of calcite under any specified conditions, e.g., in presence of calcium-ion or carbonate-ion from whatever source derived, provided only that we can ascertain what these ionic concentrations actually are.

2. The concentration of H_2CO_3 ("free" CO_2)in solution is regulated by the partial pressure (P) or proportion of CO_2 in the layer of atmosphere in contact with the solution, and conversely; and, for a given value of P, it diminishes with rising temperature, since the absorption coefficient (solubility) of CO_2 diminishes.

3. At a given temperature the total solubility as usually measured —i.e., the total concentration of calcium in the solution—varies with the concentration of H_2CO_3 (hence with P), owing to the fact that the latter determines the proportion of carbonate-ion $CO_3^{=}$, hydrocarbonate-ion HCO_3^{-}, and hydroxide-ion OH^{-} in accordance with definite mathematical expressions; and since the product $[Ca^{++}]\,[CO_3^{=}]$ remains constant $[Ca^{++}]$ must vary inversely as $[CO_3^{=}]$. The presence of other salts also affects this total solubility; so long as pure calcite is the stable solid phase in equilibrium with the solution, the magnitude of this effect is readily calculable, since the several concentrations always adjust themselves until the solubility-product $[Ca^{++}]\,[CO_3^{=}]$ attains its characteristic value.

4. The solubility-product constant of calcite diminishes with rising temperature; it is not affected to an appreciable extent by change of *hydrostatic* pressure.

The mathematical expressions are given below:

$$[H_2CO_3]=cP$$
$$[Ca^{++}]\,[CO_3^{=}]=K_C \text{ (in presence of solid calcite)}$$
$$[HCO_3^{-}]^2/[CO_3^{=}]=l[H_2CO_3] \quad =lcP$$
$$[OH^{-}]^2/[CO_3^{=}]=m/[H_2CO_3]=m/cP$$

where c, K_C, l, and m are constants at any given temperature.[2] We may note, moreover, that the free CO_2 and the total CO_2 (i.e.,

[1] Similar remarks apply, *mutatis mutandis*, to impure calcite or to aragonite; to this point we revert later.

[2] For their values and significance, see Johnston, *op. cit.*, p. 2011.

$[H_2CO_3]+[CO_3^=]+[HCO_3^-]$) determine $[OH^-]$, the degree of alkalinity (or acidity) of the solution; and that no change can be made in any one of these quantities without affecting each of the others.

Accordingly the solubility of calcite is significant only if the concentration of free CO_2 is controlled and measured, for changes in the latter, such as may easily occur, exert a large influence on the amount dissolved.[1] This is evident from Table I, which gives the solubility

TABLE I

SOLUBILITY OF CALCITE AT 16° FOR VARIOUS VALUES OF P

CO_2 IN THE ATMOSPHERE EXPRESSED		FREE CO_2 OR H_2CO_3 IN SOLUTION, PARTS PER MILLION	SOLUBILITY OF CALCITE, PARTS $CaCO_3$ PER MILLION
As Partial Pressure P	As Parts per 10,000 (by Volume)		
0.0001	1.0	0.18	44
.0002	2.0	.36	55
.00025	2.5	.45	59
.0003	3.0	.55	63
.00035	3.5	.64	66
.0004	4.0	.73	69
0.0005	5.0	0.90	75

at 16° for various values of P not far removed from the proportion normally present in the atmosphere (about 3 parts per 10,000). Calculation shows that except for very small partial pressures of CO_2 the calcium in solution is associated almost entirely with bicarbonate—thus even when P is only 0.0005, the proportion as carbonate is only about 2 per cent, whereas when P is 1.0, the proportion is less than 1 part in 30,000; nevertheless, carbonate is still the solid phase which separates out, an excellent example of the fact that it is the solubility relations and not the "affinity" relations in solution that determine which of the possible stable solid phases shall appear.

[1] Neglect of this factor or, in general, a failure to secure equilibrium conditions is responsible for erroneous statements in the literature. For instance, the solubility as given by Treadwell and Reuter (*Z. anorg. Chem.*, XVII [1898], 170) is not a real solubility at all; acceptance of their figure (238 parts per million) has led several writers astray. Cf. *op. cit.*, p. 2009. Thus on this basis J. C. Jones (*Science*, XX [1914], 829) concluded that the waters of the Lake Lahontan basin are only about one-twentieth saturated with $CaCO_3$.

The change of solubility with temperature, the proportion of CO_2 being constant, is evident from Table II, which contains values interpolated from the curve expressing the observations by Wells,[1] as well as the molar absorption-coefficient (c) of CO_2 and the calculated value (K_C') at each temperature. There is a slight error involved in identifying K_C' with the solubility-product constant $[Ca^{++}]\,[CO_3^{=}]$ except at temperatures close to 18°, because in calculating K_C' we have—for lack of better knowledge—assumed that the ratio (nr) of the first to the second ionization-constant of H_2CO_3 is independent of the temperature; nevertheless, since these values of K_C' were obtained from actual measurements of solubility, they enable one to calculate[2] for any temperature up to 30° the solubility of calcite under any conditions of CO_2 pressure or salt-concentration.[3]

TABLE II

THE SOLUBILITY OF CALCITE UNDER ATMOSPHERIC CONDITIONS (P=0.00032), AND THE SOLUBILITY-PRODUCT CONSTANT AT SEVERAL TEMPERATURES

Temperature t	Solubility of Calcite Parts $CaCO_3$ per Million	Molar Absorption Coefficient of CO_2 at t c	Solubility-Product Constant $K_C' \times 10^8$
0	81	0.0765	1.22
5	75	.0637	1.14
10	70	.0535	1.06
15	65	.0455	0.99
20	60	.0392	0.93
25	56	.0338	0.87
30	52	0.0297	0.81

From the foregoing it follows that in order to decide definitely if a natural water is saturated with respect to calcite one must know: (*a*) the concentration of free CO_2 in the water, (*b*) the temperature, (*c*) the concentrations of the other constituents present. Of these the third is the only one which has in general been satisfactorily ascertained, but it is only of subsidiary importance; experimental data on the two important factors are commonly either lacking or

[1] R. C. Wells, *Jour. Wash. Acad. Sci.*, V (1915), 617.

[2] For the mode of calculation see Johnston, *op. cit.*, p. 2011.

[3] This holds only so long as *calcite* is the stable phase. If the salt-concentration is such that some other carbonate (e.g., a double carbonate) is the stable phase, the appropriate constant must be employed in place of that characteristic of calcite.

untrustworthy.[1] On the other hand, the concentration of free CO_2 in any water, at a given temperature, can be calculated by means of the known absorption coefficient of CO_2, if the proportion of CO_2 in the atmosphere with which it has been in contact is known;[2] and as at the present time this proportion is usually close to 3 parts in 10,000 the corresponding solubility of calcite in natural waters should be close to the values given in Table II. Consideration of the published analyses[3] from this standpoint leads to the conclusion that the surface layers of the warmer portions of the sea (in so far as they have been investigated), as well as many river waters,[4] are substantially saturated with calcite. Murray,[5] in adverting to this question, states the opinion that "the ocean as a whole remains just about saturated for calcium carbonate"; but this statement is without doubt too sweeping, except in the sense that the concentration of $CaCO_3$ throughout the ocean is probably as great as it is in the warm surface layers. But there is also more direct evidence. Thoulet[6]

[1] The titration methods which have usually been employed for the determination of free CO_2—and to some extent of combined CO_2—are altogether untrustworthy, since the results depend upon the amount of indicator added and upon other factors which have not been adequately controlled. This question is discussed at length in another paper (J. Johnston, *Jour. Am. Chem. Soc.*, XXXVIII [1916], 947). Cf. also Morgulis and Fuller, *Jour. Biol. Chem.* XXIV (1916), 31.

[2] With regard to the solubility of CO_2 in a sea-water see C. J. J. Fox, *Trans. Faraday Soc.*, V (1909), 68.

[3] See F. W. Clarke, *Data of Geochemistry;* but especially a paper by E. Dubois, "The Amount of the Circulation of $CaCO_3$ and the Age of the Earth." (*Proc. Acad. Wetenschappen Amsterdam* [1901], pp. 43–62). Cohen and Raken (*ibid.* [1901], p. 63) have determined directly the solubility of $CaCO_3$ in an artifical sea-water at 15° and found about 55 parts per million; but their method of experiment is not unexceptionable and would tend to yield low results; they also conclude that this sea-water is saturated with $CaCO_3$. Wells also (*Jour. Wash. Acad. Sci.*, V [1915], 621) points out that the amount of carbonate carried by the Mississippi River diminishes steadily as it flows southward, i.e., in the direction of rising temperature.

[4] Indeed, the amount carried by many rivers is much in excess of the true solubility, indicating that some of it is in suspension. Where such a river reaches the sea, the salts cause the flocculation of this and any other suspended material, and in this way induce the formation of deposits there.

[5] Murray and Hiort, *The Depths of the Ocean* (1912), p. 180.

[6] J. Thoulet, "Etude bathylithologique des côtes du Golfe de Lyons," *Annales de l'Institut Océanographique*, IV, fasc. VII (1912), pp. 32–35.

studied silt grains taken from various parts of the Gulf of Lyons, and observed upon them films of calcium carbonate which had been precipitated during the process of sedimentation; this shows, therefore, that the water of the Gulf of Lyons is substantially saturated with $CaCO_3$. Recent experiments of A. G. Mayer[1] show that the sea-water about the coast of Florida is likewise substantially saturated, for shells exposed to it for a year lost no significant weight. Moreover, the investigations of T. W. Vaughan[2] on coral reefs "show that submarine solution is not effective there [about Florida] as all the bays, sounds, and lagoons are being filled with sediment," a conclusion which accords "with the conclusions reached by numerous investigators in the Pacific, which are that the more or less continuous walls inclosing lagoons have been formed by constructional geologic processes and that lagoon channels and atoll lagoons are not due to submarine solution."

The evidence just presented leads us, therefore, to the opinion that the surface layers of the ocean, except in the polar regions and within currents of cold water—in other words, the warmer portions of the ocean water—are substantially saturated with $CaCO_3$. We wish to point out specifically, however, that this belief cannot be regarded as established (or indeed disproved) until trustworthy determinations of the several quantities concerned have been made; indeed, to emphasize the necessity of such investigations is the prime purpose of this paper. But in this connection it may be remarked that a *permanent* deposit of limestone can hardly result unless (1) the *final* solution locally in contact with it is saturated, or (2) the precipitated carbonate is protected from the water by an organic tissue or otherwise, or (3) the process of deposition is rapid, in water circulating very slowly or not at all, under which conditions re-solution by diffusion is very slow.

In this paper, which is dealing primarily with the chemical arguments, it would be out of place to take up the geologic lines of evidence which indicate that the ocean as a whole is not saturated with $CaCO_3$, for this point is not at issue; but we may fitly advert to the

[1] A. G. Mayer, *Proc. Nat. Acad.*, II (1916), 28.

[2] T. W. Vaughan, *Am. Jour. Sci.*, XLI (1916), 133. See also his earlier papers, especially in the *Year Books* of the Carnegie Institution of Washington.

chemical arguments which have been adduced in favor of this position. Thus Tolman writes:[1]

> As direct evidence that the ocean is not saturated with calcium acid carbonate, we find (1) of the many hundred bottles of the Challenger's samples of sea-water, from all depths and collected at all temperatures, kept several years, only one or two showed deposit of lime.[2] (2) Sea-shells from the bottom of the Pacific show corrosion and re-solution.[3] The Pteropod shells are not found below fifteen hundred fathoms, and two thousand eight hundred fathoms is the limit for the globigerina ooze.[4] (3) Thoulet found by actual experiment that sea-water will dissolve calcium carbonate from shells, corals, etc.[5] (4) Usiglio, studying the evaporation of the Mediterranean water at Cette, found that no precipitate was formed until the specific gravity of the sea-water increased from 1.02, the specific gravity of the unevaporated water, to 1.0503, when the first precipitation begins, composed largely of calcium carbonate with ferric oxide.[6]

Let us now consider these arguments severally. (1) That samples of surface water did not deposit $CaCO_3$ on standing is not good evidence one way or the other unless conditions were carefully controlled, for change of temperature or of concentration of CO_2 would influence the result. (2) This shows, of course, that the lower (and colder) layers are not saturated; to this point we revert later. (3) Reference to Thoulet's paper shows that his work proves nothing as to the point in question, for neither the temperature nor the partial pressure of CO_2 was controlled. Indeed, he writes: "In the case of marble . . . . and of coral, the loss [of weight] in sea-water was *negative*. This result arises from the fact that small algae appeared . . . . the weight of which confuses the result." (4) This observation also is no proof, in view of the well-known fact that solutions of calcium carbonate exhibit a great tendency to supersaturation when no solid $CaCO_3$ is already present; therefore it shows only that, when the density was 1.05, the degree of supersaturation had become so great that precipitation took place. From

[1] C. F. Tolman, *Jour. Geol.*, VII (1899), 604.

[2] *Challenger Reports*, p. 221.

[3] *Jour. Geol.*, I, 504.

[4] *Challenger Reports*, p. 221.

[5] *Comptes rend.*, CVIII, 753.

[6] *Encycl. Brit.*, XXI, 229.

this discussion, then, it follows that three of these arguments are not conclusive as to the point at issue.

Let us now consider the modes in which $CaCO_3$ may be precipitated. We shall for convenience arrange them under three heads, which, however, cannot be sharply differentiated: (1) direct evaporation of the water; (2) through organic agencies; (3) change of conditions, especially of temperature and concentration of free CO_2, these being the predominant inorganic factors.

1. *By direct evaporation.*—When natural waters evaporate, $CaCO_3$ is commonly (though not necessarily) the first substance to be deposited, and may be very largely precipitated before any of the other salts separate;[1] the more soluble salts, moreover, will tend to be leached out of such deposits. But since all such deposits are of obvious origin and of minor importance, they need not detain us further.

2. *Deposition through organic agencies.*—The agencies which come under this category are of the greatest importance and are predominantly responsible for the deep-sea deposits, yet little as to their mode of action can be definitely stated until more is known about the biologic processes involved. This question is altogether beyond the scope of this paper; we shall mention merely two established effects of organic agencies, reverting to them later: viz., the abstraction of free CO_2 from fresh water by growing plants,[2] and the production of ammonia in sea-water by decaying organisms or by bacteria.[3] Both of these effects disturb the equilibrium in a solution originally saturated with $CaCO_3$, the former by diminishing the concentration of H_2CO_3, the latter by increasing the concentration

[1] Cf. Van't Hoff's *Ozeanische Salzablagerungen.*

[2] Various references to these effects are given in Clarke's *Data of Geochemistry* under "Limestone." See especially C. A. Davis, *Jour. Geol.*, VIII (1900), 485, 494; IX (1901), 491. According to Murray, the calcareous algae common in the warmer oceans no doubt secrete their skeletons in the same way. See also S. T. Powell, "Effect of Algae on Bicarbonates in Shallow Reservoirs," *Jour. Am. Waterworks Assoc.*, II (1915), 703.

[3] This question has been discussed recently by G. H. Drew (*Carnegi ,Institution Publ. No. 182* [1914], p. 7), and by Kellerman and Smith (*Jour. Wash. Acad. Sci.*, IV [1914], 400), and so need not be treated here. Many decaying organisms and bacteria (as well as the respiration of animals) produce CO_2, and to this extent they would act as an adverse influence on the deposition of $CaCO_3$.

of $CO_3^=$ directly,[1] the net result in either case being the precipitation of an amount of $CaCO_3$ which could readily be calculated by means of the equilibrium equations if the amount of CO_2 abstracted, or of ammonia produced, were known. But by this we do not imply that an organism cannot secrete calcium carbonate except from a solution already saturated with it. Nevertheless the possibility is open that the effects just considered may sometimes be in reality examples of the changes in conditions to be next considered—that the organism may be merely the agency which localizes the process, the mechanism which occasions the precipitation. It may even be that certain bacteria are abundant where $CaCO_3$ is being precipitated because there they can easily secure material—particularly CO_2—needed for their life-processes; on this basis they would be concomitants, rather than causes, of the deposition of the carbonate.

3. *Change of conditions.*—The important physico-chemical factors are temperature, and concentration of free CO_2, of the water; in comparison with these two all other such factors are entirely subsidiary. As an illustration of the magnitude of the effect producible by change of these factors, a change in the proportion of CO_2 in the air in actual contact with the solution from 3.3 down to 3.0 parts per 10,000—a change[2] which may occur at the present time—of itself decreases the solubility from 65 to 63 parts per million, and so will cause the ultimate precipitation of the corresponding quantity of $CaCO_3$ from a solution already saturated with it. A similar amount will be deposited[3] if the temperature of the saturated solution rises about 2° C., the proportion of CO_2 in the air remaining constant; under these circumstances the concentration of free CO_2 in the water falls, and its diminution is responsible for the larger part of the diminished

[1] The production of free ammonia causes an increase in the concentration of OH^- and therefore an increase in $[CO_3^=]$, since the quotient $[OH^-]^2/[CO_3^=]$ is constant when P is constant, in accordance with the equation already given. It may be noted that the production by the decaying organisms of an ammonium salt such as NH_4NO_3 or $(NH_4)_2SO_4$ would tend to increase the solubility of $CaCO_3$ and hence would not favor its deposition.

[2] The range of variation in the course of geologic time has in all probability been very much greater than this with correspondingly larger possible consequences.

[3] This of course implies that supersaturation does not take place; but in the sea supersaturation is highly improbable by reason of the great abundance of appropriate nuclei always present.

solubility, since the diminution of the solubility-product constant of calcite with temperature is, as we have seen, proportionally less than that of the absorption coefficient of CO_2.

The abstraction of CO_2 from a saturated solution results ultimately, then, in the deposition of $CaCO_3$, no matter what the agency which abstracts the CO_2. This agency may be a diminished proportion of CO_2 in the air, or a higher temperature, or both; or it may be organisms which make use of the CO_2 in their vital processes, or the production by bacterial action of ammonia, which indirectly achieves the same result; or, in short, it may be any way in which the concentration of CO_2 may possibly be diminished. Consequently, if the surface layers of the sea are saturated, as we believe they are, precipitation of $CaCO_3$ will be brought about wherever any of the foregoing agencies are operative.

In this connection two points which are the consequence of accepting erroneous chemical data are to be noted. Thus Davis[1] in his excellent work on marls has made a slight slip. He observed that on bubbling oxygen gas through a solution containing $CaCO_3$ the latter was precipitated, and he attributed this effect to a specific action of the oxygen; but any other gas would have produced the same effect, which was actually due to the sweeping out of the CO_2 from the solution. Nor is it necessary to consider, when ammonia is being produced, whether it appears as hydroxide or as carbonate, or whether there is a subsequent metathesis with calcium sulphate or chloride or some other reaction; in either case the net result can be predicted immediately from a consideration of the effect of the added substance upon the concentration of calcium-ion and of carbonate-ion,[2] and of the magnitude of the product $[Ca^{++}]\,[CO_3^{=}]$ in relation to its precipitation value.[3] To some this procedure may appear complicated; in reality, while it pays no heed to those easily derived arithmetical equations so often considered as representing reactions, it takes into account the several equilibria which must be adjusted,

[1] C. A. Davis, *Jour. Geol.*, VIII (1900), 487.

[2] Change of concentration of $CO_3^{=}$ affects, and is affected by, the concentration of HCO_3^{-} and OH^{-}, these being all dependent variables; see Johnston, *op. cit.*

[3] Provided that calcite is still the stable solid phase in equilibrium with the solution; cf. footnote 3, p. 733.

and is the only procedure which will yield accurate results and lead to correct conclusions. Moreover, a comprehension of this question is desirable because this apparently complex equilibrium is typical of what takes place in many other systems, aqueo-igneous and igneous as well as aqueous; it is in but very few cases, however, that we know even what molecular species are important factors in the equilibrium, and in still fewer is any information available as to the quantitative relations at equilibrium.

There has, moreover, been considerable misapprehension as to the rôle of hydrostatic pressure in increasing the solubility of $CaCO_3$; thus in a recent paper Daly[1] writes: "On account of the higher temperatures and lower bottom pressures (*pressure increasing the solubility of the carbonate*) of the shallower water we should expect the rate of chemical precipitation of calcium carbonate at the bottom to be concentrated in the neritic (epicontinental) and shallower bathyal regions." And many smiliar statements relative to the effect of hydrostatic pressure might be quoted. As a matter of fact, the hydrostatic pressure[2] acting on the water is of itself an absolutely negligible factor; thus water a mile below the surface of the sea will hold in solution an amount of $CaCO_3$ which does not differ by an appreciable quantity from the amount the same water at the surface will hold, provided that the concentration of free CO_2 and the temperature be the same in both cases. The increased solubility with depth in the ocean is due entirely to the lower temperature of the water and to the increased proportion of free CO_2, but not at all to the increased hydrostatic pressure there prevailing.

The only pressure which does affect the solubility is the partial pressure, i.e., the proportion, of CO_2 in the layer of atmosphere[3] in

[1] R. A. Daly, *Bull. Geol. Soc. Am.*, XX (1909), 156; also in "Geology of North American Cordillera," *Memoir No. 38, Geol. Survey of Canada*, Part II (1912), p. 651. Italics are ours.

[2] Increase of hydrostatic pressure *decreases* the solubility of some substances. In any case the effect is very small indeed; its magnitude and direction can be calculated if the appropriate data on volume changes are known. Cf. Johnston and Niggli, *Jour. Geol.*, XXI (1913), 504, where references are given.

[3] The proportion of CO_2 in the air increases, *ceteris paribus*, as we pass from higher to lower levels; but this is a factor of no moment to the present discussion because the diffusion downward through the water is in all probability very slow in comparison with the natural circulation of the water.

contact with the solution, for there is a definite and quickly attained equilibrium between the proportion of CO_2 in the adjacent atmosphere and the concentration of free CO_2 in the water, the factor of proportionality being the absorption coefficient (the solubility) of CO_2 in the solution at the particular temperature. It is true that water at depth can hold more CO_2 in solution, if it gets hold of it, for in that case bubbles of CO_2 gas cannot form until its virtual pressure just exceeds the hydrostatic pressure; but slow diffusion upward would tend to equalize the concentration at various depths. In the ocean, on the other hand, the content of CO_2 is only what it was able to absorb when at the surface, supplemented by that which has been produced by organic processes—the latter being in all probability but a small fraction of the whole in deep water.[1] However this may be, it is manifest why the water at depth should contain more CO_2, for its present low temperature, retained from its polar days, establishes the fact that when at the surface in contact with the atmosphere it was cold, and lowering of temperature increases very markedly the amount of CO_2 which water can absorb through contact with an atmosphere containing a constant proportion of CO_2.[2] This fact, combined with its present low temperature —for, as we have seen, lowering of temperature of itself increases the solubility of $CaCO_3$—suffices to account for the well-known fact that all shells and tests disappear in the depths of the ocean.[3]

Now let us revert to the consequence of abstraction of CO_2, and consider what will happen when, in the course of the oceanic circulation, this cold water, which carries more CO_2 and more $CaCO_3$ than the warmer surface waters,[4] reaches the surface and is slowly warmed.

[1] Buchanan (*Proc. Roy. Soc.*, XXII [1874], 483) writes: "Down to nearly 2,000 fathoms life is still abundant; below this depth, however, the amount rapidly decreases till, at about 2,800 fathoms, it is, for carbonic acid producing purposes, practically extinct."

[2] Thus in contact with any atmosphere, water (or a dilute salt solution) absorbs about twice as much CO_2 at 0° as at 20°.

[3] See the report of the Challenger expedition or the work of Sir John Murray. In the present connection it is immaterial whether these shells consist of calcite or aragonite, although assertions to the contrary may be found.

[4] This appears a necessary prerequisite, no matter what be the mechanism of precipitation. Dittmar, in his article in the *Encyclopaedia Britannica*, states that there is a slight but indubitable increase in concentration of calcium with depth. Moreover,

In the first place, it will gradually lose CO_2 to the air, the residual concentration of free CO_2 being dependent at any moment upon the temperature of the water and the proportion[1] of CO_2 in the air at that place. The consequence of this loss is that the amount of calcium in solution will at some point exceed the concentration which the water is able to hold in solution—or, in other words, the product $[Ca^{++}][CO_3^{=}]$ reaches its characteristic precipitation value—whereupon precipitation[2] sets in, and continues thereafter so long as the temperature continues to rise. This process is without doubt taking place now in tropical and subtropical regions wherever and whenever the necessary conditions are fulfilled. It has been correlated[3] with the abundant bacterial and planktonic life found under such circumstances, and there would seem to be little question that the organisms are a factor in the process, if only in the sense of catalyzing it. But may it not be, in some cases at least, that the organisms are abundant there because of the abundance of the CO_2 available for their life-processes in such water? For it is to be borne in mind that the precipitation of $CaCO_3$ is accompanied by the setting free of an equivalent quantity of CO_2 which, if not used up in the sea, will pass into the atmosphere. Be this as it may, the physico-chemical factors are in themselves competent to account for the precipitation[4] of $CaCO_3$ on a large scale, and the prerequisite conditions for deposition by this means do not differ materially from the postulates required for precipitation by bacterial action or by organisms generally.

Buchanan (*Proc. Roy. Soc. London*, XXIV [1876]) writes: "There is usually more CO_2 in waters taken from the bottom and intermediate depths than in surface water; but if regard be had to the temperature of the water, it will be seen that there is but little difference in the amount in waters of the same temperature from whatever depth they have been derived." It is to be observed that these determinations all refer to low latitudes; conditions in the Polar regions may well be different.

[1] All experiments indicate that this proportion departs in general very little from 3 parts in 10,000, except in or near large towns. Off the west coast of Greenland, however, amounts up to 7 parts in 10,000 were observed by Krogh (*Meddelelser om Gronland*, XXVI [1904], 409).

[2] Supersaturation is under these conditions obviously a negligible factor.

[3] G. H. Drew, *Carnegie Inst. Publ. No. 182* (1914), p. 7; Kellerman and Smith, *Jour. Wash. Acad. Sci.*, IV (1914), 400. See also recent papers by T. Wayland Vaughan.

[4] Likewise for its re-solution.

Indeed, there are several facts which point to a parallelism between the amount of lime secreted by organisms and the degree of saturation of the sea with respect to $CaCO_3$; thus the animals of the warm seas secrete more lime, on the average, than the same types in cold seas;[1] and, according to Murray,[2] "on the whole, lime at the present time appears to be accumulating toward the equator." These observations directly corroborate the idea that solubility is a significant factor even in the secretion of lime by organisms; that the decreasing abundance of calcareous organisms toward the polar regions is a question not only of the decrease of general vitality (rate of growth and of reproduction) with lowering of temperature, but also of the decreasing capacity[3] of the organism to secrete $CaCO_3$ from colder sea-water, this being associated with the fact that, though the concentration of lime is no smaller in the colder water, the degree of unsaturation is greater the colder the sea-water.

According to Murray, "a limited amount of purely inorganic precipitation does, indeed, take place in coral reefs and some shallow water deposits and in the Black Sea."[4] Now it has been argued[5] that chemically precipitated limestones are due to the production of ammonia by decaying organic matter; according to this view such limestones could form only when conditions were such that a long-continued process of persistent decay was possible. According to the view emphasized in the present paper—and, be it noted, this is primarily a chemical, rather than a geological, question—chemical

[1] See citations from the *Challenger Reports*, in Chamberlin, *Jour. Geol.*, VII (1899), 576–77.

[2] *The Depths of the Ocean*, p. 180. "In very deep water, even within the tropics, the calcareous shells do not accumulate on the bottom, being apparently removed through the solvent action of sea-water, and with increasing depth the Globigerina ooze passes gradually into another pelagic type, usually Red Clay" (p. 164). "Pteropod ooze is limited to the tropical and subtropical regions, usually in the neighborhood of oceanic islands and on the summits and sides of submarine elevations; it is found in relatively shallow water, and covers a relatively small extent of the ocean floor" (p. 167).

[3] It would be of interest to know if these calcareous organisms could secrete $CaCO_3$ from colder water kept saturated with calcium carbonate.

[4] *The Depths of the Ocean*, p. 178.

[5] Most recently by R. A. Daly, *Bull. Geol. Soc. Am.*, XX (1909), 153; in more extended form in *Memoir No. 38, Geol. Survey Canada* (1912), pp. 643 f.

precipitation would take place wherever, and so long as, a current of water saturated with calcite was being warmed. These views are not at all mutually exclusive; but their implications differ, and it ought to be possible to decide by appropriate observation and deduction in how far either has been a dominant cause on a large scale.

The magnitude of the scale of this presumed process of precipitation through purely inorganic agencies depends primarily upon the rate of circulation and upon the amount of calcium carried by this water rising to the surface. We shall now consider the competence of these agencies as geologic factors. In doing so let us suppose that a cold current of sea-water is not saturated with $CaCO_3$ until it has reached a temperature of 15°, and that this current after traveling 1,000 kilometers (600 miles) has attained a temperature of 20°; further, that the water in this stretch of 1,000 km. is changed 10 times a year, corresponding to a current speed of $1{,}000 \times 10/365$ or 27.4 km. a day. Now this water in being warmed from 15° to 20° would precipitate 5.4 parts $CaCO_3$ per million by weight, or 2 parts per million by volume; on these assumptions,[1] therefore, in the course of a year the mean thickness of the deposit (presuming that all of the precipitate finally settles to the bottom within this stretch) would be 2/1,000,000 of the depth of the current. Hence if the depth of the current is 100 m., the average deposit over the whole area would be, on the specific assumptions just mentioned, 2 mm. yearly.[2] This estimate is probably a minimum, particularly because we have supposed that deposition would take place over the whole area, whereas in reality deposition would be localized (e.g., if there is, as is likely, a more rapid warming up at some places) so that the deposits actually formed would be thicker. Moreover, the deposit would be redissolved whenever the current is underlain by colder unsaturated water; therefore actual deposits belonging to this category should occur only in localities bathed by currents which are

[1] The numerical values adopted were chosen as being reasonable; in any case these calculations will serve as an illustration, and anyone may make similar calculations using whatever numerical values he deems most consonant with the facts.

[2] This corresponds to about 5,000 tons $CaCO_3$ per square kilometer per year, or to a thickness of about 8 inches in a century.

warm and hence comparatively shallow and rising in temperature as they proceed. Consequently, if deposits of $CaCO_3$ are being formed in this way—and there is no direct evidence at hand which contradicts this view—it should be possible to correlate the position and rate of formation of such precipitated deposits with other things by means of series of bathymetrical observations on the manner of flow of the currents, the temperature of the water, and, above all, the concentration of free CO_2. As regards the latter, it may be said that the methods hitherto in vogue are very faulty indeed,[1] and that systematic, accurate determinations of the free CO_2 (which can be made by proper choice of method) are very much to be desired, not only on account of their bearing on the present question, but because an accurate knowledge of the concentration of free CO_2 is of high importance[2] in connection with many biological problems, both theoretical and practical.

Nor would the establishment of this presumed correlation between deposits of $CaCO_3$ and the physico-chemical conditions prevailing in the ocean be of importance only in relation to present-day formations belonging to this category; it would also be of use in interpreting past deposits of this character which have persisted and in co-ordinating them with other factors. For the rate of formation of such deposits (including the limiting case of zero rate) depends obviously upon the mode and rate of circulation of the ocean and the amount of calcium carried by the water rising to the surface, secondarily, therefore, upon the amount of calcium carbonate brought down by the rivers to the sea; all of which depend ultimately upon the physiographic and petrologic character of the land surface, upon the magnitude of the seasonal variations and regional differences, upon the climate over the whole earth, and upon the proportion of CO_2 in the atmosphere. It would lead too far to discuss this question in all its bearings;[3] in order to show the importance of the

[1] See Johnston, *Jour. Am. Chem. Soc.*, XXXVIII (1916), 947.

[2] The free and combined CO_2 and the alkalinity of the solution are not independent variables, a fact often forgotten; and doubtless many effects ascribed to a change in alkalinity are due equally, or primarily, to a change in the CO_3 equilibrium in the solution.

[3] The discussions of Chamberlin (*Jour. Geol.*, VII [1899], 545, 667, 757), Tolman (*ibid.*, p. 585), Krogh (*Meddelelser om Gronland*, XXVII [1904], 334), and others, require some revision in the light of data available since that time.

last factor—apart altogether from the influence of CO_2 as an agency disintegrating the rocks—we have calculated the concentration of the free and combined CO_2 in sea-water at three temperatures for several proportions of CO_2 in the atmosphere. The specific assumptions made in these calculations are: (*a*) that the molar absorption coefficient (c) of CO_2 is the same as in a 0.6 N (3.5 per cent) solution of sodium chloride; (*b*) that the water is always saturated with respect to calcite, so that we are justified in using the solubility-product constant (K'_C) corresponding to the temperature; (*c*) that the degree of ionization of the carbonate is 0.6, a value which is probably high rather than low.[1] On this basis the formula becomes

$$A = \text{total } CO_2 = cP + \sqrt{11200cK'_CP}/0.6,$$

where the first term represents the free CO_2 and the second the total combined CO_2, each expressed in moles per liter; whence by multiplication by the factor 0.044 one obtains the result in grams CO_2 per cubic meter (parts per million) as given in Table III.

TABLE III

THE CONCENTRATION OF FREE (f), COMBINED (b), AND TOTAL (A) CO_2—EXPRESSED IN GRAMS CO_2 PER CUBIC METER (PARTS PER MILLION)—IN SEA-WATER AT SEVERAL TEMPERATURES AND SEVERAL PARTIAL PRESSURES (P) OF CO_2 IN THE ATMOSPHERE; CALCULATED ON THE BASIS OF THE SPECIFIC ASSUMPTIONS MENTIONED ABOVE

CO_2 IN ADJACENT ATMOSPHERE AS		10° c=0.0463 K_C=1.06×10^{-8}			20° c=0.0335 K_C=0.93×10^{-8}			30° c=0.0260 K_C=0.81×10^{-8}		
Parts per 10,000	Partial Pressure P	f	b	A	f	b	A	f	b	A
2.5	0.00025	0.51	81.5	82	0.37	70.0	70	0.29	61.5	62
3.0	.0003	0.61	86.6	87	0.44	74.5	75	0.34	65.5	66
3.5	.00035	0.71	91.1	92	0.51	78.3	79	0.40	68.8	69
30.0	.003	6.12	187.6	193	4.4	160.3	165	3.43	140.8	144
300.0	0.03	61.2	402.	463	44.0	345.	389	34.3	303.	337

The figures for total CO_2, derived in this way, are in substantial agreement with the results of analyses of sea-water; in any case the

[1] Murray and Hiort (*The Depths of the Ocean* [1912], p. 175) estimate the aggregate degree of ionization in sea-water to be 0.9; but this is undoubtedly much too high.

relative values for the different conditions are probably good even if the absolute values are inaccurate. According to the table, the concentration of total CO_2 in water at constant temperature varies practically as the cube root of P, for the small values of P; in other words, a change of 3 per cent in the present proportion of CO_2 (e.g., from 3.1 to 3.0 parts per 10,000) will produce a change of but 1 per cent in the concentration of total CO_2 in the sea-water. Likewise under present conditions (i.e., $P=0.0003$) the total CO_2 in the ocean decreases about 1.5 per cent of its value for each degree of rise in temperature. At the higher pressures of CO_2 the proportion of free CO_2 in the water becomes relatively much more important; but a hundred-fold increase in the proportion in the adjacent air would cause only a fivefold increase in the total CO_2 in the sea. In this estimate and in the subsequent discussion, be it noted, the assumption is implicit that the water is continuously saturated with $CaCO_3$ at 15° for all values of P, which in turn implies that conditions were such that the rivers transported to the sea sufficient lime to achieve this. On this basis, therefore, if the present amount of CO_2 in the atmosphere were increased a hundred fold, the total amount of CO_2 in atmosphere and ocean would be only six times as much as it is now; the conditions of equilibrium always being such that a chance in the proportion of CO_2 in the atmosphere is minimized, not through a permanent change in the proportion of free CO_2 in the sea (and of its alkalinity), but ultimately by means of the precipitation or solution of a definite quantity of $CaCO_3$.

Let us now make a computation of the ratio of the total amount of CO_2 in the whole ocean to that in the whole atmosphere, this being, of course, a measure of the capacity of the ocean to regulate the proportion now present in the atmosphere. We assume again that the ocean as a whole would be saturated with $CaCO_3$ if its temperature were 15°,[1] and that its mean depth is 3,600 meters;[2] on this basis the mean amount of CO_2 under each square meter of surface of the sea is $81 \times 3{,}600$ gm. or 290 kg. The mean amount above each square

[1] This is just equivalent to the assumption that the average proportion of $CaCO_3$ throughout the ocean is that which corresponds to its solubility at 15°, about 60–70 parts $CaCO_3$ per million for values of P not far removed from 0.0003. Cf. Table I.

[2] See *Encycl. Brit.*, article "Ocean."

meter of the earth's surface (sea and land together) is 3 kg.; for if the proportion of CO_2 in the air at the earth's surface is 3 parts per 10,000, the proportion in the whole atmosphere is 2 in 10,000 by volume, hence 3 in 10,000 by weight, or 3 kg. per square meter. Consequently, since the ocean covers about 71 per cent of the total surface of the globe, the ratio

$$\frac{\text{total } CO_2 \text{ in ocean}}{\text{total } CO_2 \text{ in air}} = \frac{290 \times 0.71}{3} = 69.$$

In other words, the ocean contains about 70 times[1] as much CO_2 as the air, on the basis of the assumptions specified above. On this basis the total CO_2 now present in the ocean and atmosphere combined would form a layer of $CaCO_3$ only about 17 cm. thick over the whole globe, or about 86 cm. (nearly 3 feet) over one-half the present land area; likewise if the amount of CO_2 in the atmosphere were 100 times as much as at present, the corresponding values would be slightly more than 6 times as large, namely, 110 cm. over the globe, or 550 cm. (18 feet) over one-half the present land area. The possible deductions, however, must remain uncertain until series of simultaneous accurate determinations of free and total CO_2, temperature, and salinity in the sea at various depths and in different localities shall have been made.

The precipitation of $CaCO_3$ in forms other than calcite.—Besides calcite, which is the stable crystalline form of $CaCO_3$ under all ordinary conditions, there are two unstable crystalline forms, aragonite and μ-$CaCO_3$, which may precipitate under certain circumstances. This whole question is discussed at length in another paper, to which the reader desirous of further information is referred;[2] we shall here merely recapitulate the conclusions relevant to the present discussion. The existence of the μ-form in nature has not been definitely established, possibly by reason of the fact that some of the criteria which have been used to differentiate the several forms of $CaCO_3$ have not been unexceptionable, possibly on account of its

[1] This estimate is higher than that (27 times) of Krogh (*Meddelelser om Gronland*, XXVI [1904], 420) or that (55 times) given in Chamberlin and Salisbury's *Geology*, II, 661, which see, with respect to the whole discussion.

[2] "The Several Forms of Calcium Carbonate," Johnston, Merwin, and Williamson, *Am. Jour. Sci.* (4), XLI (1916), 473.

instability, for in presence of water the μ-form transforms to calcite in a few days. Calcite also appears as spherulites and as "amorphous" $CaCO_3$; but there is little question that the divergent properties of the latter are due entirely to its fineness of grain, i.e., to its extent of surface in proportion to its mass. Consequently the only form other than calcite which we need consider here is aragonite.

Apparently aragonite is formed in nature (*a*) through organic agencies (e.g., in certain shells), (*b*) by deposition from hot springs, (*c*) when an isomorphous carbonate is present to serve as nucleus, and (*d*) by chemical precipitation in saline waters, even at ordinary temperatures, under circumstances which we are unable to specify except by saying that the presence of sulphate appears to be a favorable factor. But *pure* aragonite cannot persist for any length of time in presence of water and calcite, hence only in special circumstances will it be found persisting in the sea. There is, however, the possibility that aragonite may take up in solid solution enough material to bring its own solubility below that of calcite, and hence in the saline solution in equilibrium with the solid solution to render the latter stable with respect to calcite; on this basis it is possible that such impure aragonite may persist in contact with sea-water under certain circumstances, although when exposed to the action of meteoric waters it would soon transform to calcite. However this may be, the circumstance that $CaCO_3$ precipitates otherwise than as calcite would not of itself affect appreciably anything stated in this paper, since the whole effect would be that ensuing upon the substitution for the solubility-product constant of calcite of the corresponding value for the other form, the latter being certainly no more than twice as great as the former; so the precipitation of the less stable forms is therefore of only subsidiary importance in the present connection.

Summary.—Though organic agencies are predominantly responsible for the deposition of calcium carbonate, yet the purely inorganic factors should also be taken into account in discussions of the mode of deposition. In this paper emphasis has been laid on one point which has not received adequate recognition; namely, the concentration of calcium relative to the limiting saturation concentration of calcium carbonate under the particular conditions, or, in other

words, the relative degree of saturation with respect to calcium carbonate in the ocean. The importance of this factor is obvious if we recollect that the chance of a permanent deposit is, *ceteris paribus*, greater the more nearly saturated the surrounding water is; its neglect is doubtless due to the erroneous and misleading statements as to the solubility of $CaCO_3$ which have been prevalent. The solubility under specified conditions can now be calculated with the requisite accuracy; it is affected materially by variations of temperature and of concentration of free CO_2 such as occur in nature. For example, a change in the proportion of CO_2 in the adjacent air from 3.2 to 3.0 parts per 10,000, or an increase of temperature of 2° C. would result ultimately in the precipitation of about 2 gm. $CaCO_3$ from every cubic meter of a solution saturated with it. Comparison of the solubility as calculated with the available analytical data indicates that the warmer surface layers of the sea are substantially saturated with respect to calcite, and consequently that precipitation is to be expected wherever the water is being warmed or is losing CO_2, or both, and this independently of any other agencies. Indeed, these inorganic factors must be considered no matter what may be the agency inducing precipitation; for example, there is ground for believing that calcareous organisms are more abundant the more nearly saturated with $CaCO_3$ the water is. The view here advocated, that a somewhat greater rôle be assigned to the inorganic factors than has hitherto been usual, does not conflict with other views—it merely shifts the emphasis a little; nor does it conflict with any facts that have been definitely ascertained. Its precise importance can be determined only by accurate determination of temperature, salinity, and particularly of concentration of CO_2—free and total—of the water carried out systematically over the ocean; the results of such an investigation, properly carried out, would have an important bearing on many outstanding biological, as well as geological, problems.

NOTES ON THE STRUCTURAL RELATIONS OF AUSTRALASIA, NEW GUINEA, AND NEW ZEALAND

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Introduction
Previous Work Done and Scope of Present Work
Growth of Australasia from Pre-Cambrian to Recent Time
Evidence of the Ore Deposition
Conclusion

INTRODUCTION

The accompanying brief note is an attempt at the co-ordination of our increasing knowledge of the structural development of Australia and the neighboring islands.

The ideas given in this note are intended only as a temporary viewpoint from which to consider the work of the great pioneers of geology in Australia and as an inference or tentative hypothesis to stimulate interest in those magnificent field problems in Australasia, New Guinea, New Caledonia, and New Zealand, which call so urgently for solution. In this way it is hoped that the scheme here proposed will serve as a rough clue to the unraveling of certain vexed questions in the stratigraphic and structural history of Australia.

Several difficult points need explanation before any simple account of the building of Australia would be possible. Thus in the discussion of the Devonian it must not be forgotten that folds supposed to be of this age occur in *Northwest* Australia. Highly altered rocks of unknown age and of large area occur also in Northern and Northeastern Queensland, and the occurrence of these has not been explained in the present note. Then again, it must not be forgotten that our knowledge of some of the Permo-Carboniferous rocks, such as the Gympie of the Queensland geologists, is far from satisfactory.

Again, it is not known how many of the observations of the older workers in connection with the strike of folds were merely local and how many were conducted on a large scale.

Acknowledgments.—The writer is deeply indebted to Professor Leo A. Cotton and Dr. W. N. Benson, of Sydney University, for their kindness in reading the report in manuscript and for supplying additional information as to literature on the pre-Cambrian and Ordovician, and for kindly criticism of the notes on the Devonian, the Permo-Carboniferous and the Trias-Jura.

Previous workers.—It is the desire of the writer at this stage of our scientific development in Australia to draw attention to the work of the pioneers of geology in the great island continent. Australian pioneer geologists, in common with Australian explorers and miners, and in common also with American pioneers, have breathed the inspiration of their own mighty surroundings. Foremost among the pathfinders of geology in the country under consideration—men who crossed trackless wastes and endured untold discomforts in their pursuit of knowledge—were W. B. Clarke, R. Daintree, and A. R. C. Selwyn; others who followed in the track of these giants, but who nevertheless bore much of the heat and burden of the day and were either worthy successors or contemporaries of the pioneer trio, were H. Y. L. Brown, J. E. Carne, T. W. E. David, W. Howchin, R. Logan Jack, A. Gibb Maitland, R. Murray, S. Stutchbury, R. Tate, W. H. Twelvetrees, and C. S. Wilkinson. Among them also must be named the paleontologist, R. Etheridge, Jr., whose labors in the cause of Australian paleontology have done so much to simplify the task of the field workers. A whole group of younger enthusiasts have built and are today building on the work laid down by these pioneers.

PREVIOUS WORK DONE AND SCOPE OF PRESENT NOTE

The earliest definite statement known to the writer concerning the building of Australia as a whole was made by T. W. E. David.[1] In this detailed account Professor David's descriptions imply

[1] Presidential address on "The Growth of Australia," *Proc. Linn. Soc. N. S. Wales*, 1893, pp. 547–607.

the growth of Australia as from west to east. In 1911 the same writer amplified his earlier statement and said:[1] "Since the close of Paleozoic time Australia has been subjected to broad warps, but not to true folding except in the direction of New Guinea, where Cretaceous, and even early Tertiary, strata are highly folded. New Guinea is thus a new fold region; and even in Australia tectonic movements are newer as New Guinea is approached" (p. 59).

H. I. Jensen[2] also, in a later note, discussed the gradual growth of the eastern portion of the continent.

Like David's earlier reports, this paper of Jensen's is important and suggestive. Jensen approaches the problem of Eastern Australian history also from the viewpoint of "petrological unity." He, however, considered that the folding of the Permo-Carboniferous sediments was sporadic and had died out practically in Northern New South Wales.

In 1914 David[3] presented an epitome of Australasian geology. In this he said: "The latest folding to which the earth's crust in Australia has been subjected belongs to late Carboniferous time" (p. 256). He qualified this, however, by the statement: "The strata in the Permo-Carboniferous system are either perfectly horizontal or disposed in broad open troughs and arches. Only in the case of the strata of Drake and Underçliffe in New England and the Ashford areas [New South Wales] and the Gympie area in Queensland, are the strata of this system highly disturbed near granite intrusions" (p. 267).

The reader is referred for a consideration of this statement to the discussion of the field evidence in connection with the Permo-Carboniferous. It will then be seen how incomplete is our knowledge of the age of the sediments of Eastern Australia lying to the north of Sydney, and hence how great the need for caution to be exercised in coming to any definite conclusion as to the scheme of structure.

[1] T. W. Edgeworth David, "Presidential Address," *Proc. Roy. Soc. N.S. Wales*, 1911, pp. 15–76.

[2] "The Building of Eastern Australia," *Proc. Roy. Soc. Queensland*, July, 1911, pp. 149–98.

[3] T. W. Edgeworth David, "The Geology of the Commonwealth" (Federal Handbook), *Brit. Assoc. Adv. Sci.*, Australian meeting, 1914, pp. 241–325.

Origin and scope of present note.—The idea of writing a paper similar to the present one was conceived as far back as 1905–6, when the writer was surveying an area of folded sediments in Northern New South Wales. Previous workers had considered these beds as belonging to the older Paleozoic because they were strongly folded, whereas the beds of known Permo-Carboniferous type in Australia at that time were either horizontally bedded or only moderately domed.

David, however, in connection with these beds, had pointed out as far back as 1893: "I have, however, lately come to the conclusion that the whole of the Paleozoic sedimentary rocks of the Vegetable Creek district, provisionally classed by me as Upper Silurian or Devonian, are referable to the Gympie horizon"[1] [presumably Carboniferous.—E. C. A.].

During the progress of the survey these beds were discovered to contain many characteristic Lower Marine (Permo-Carboniferous) fossils, as probably also some Upper Marine types. The area of these Permo-Carboniferous types was proved to extend far to the north and west afterward by the field work of Carne and the writer.[2]

In Southern Queensland, near Warwick, these two observers found Lower Marine rocks folded in the most complicated manner,[3] while in 1908 during a visit to Mount Morgan, about 500 miles north of the New South Wales border, they saw rocks indicated on the Queensland geological map[4] as of the same age as the Warwick types, also highly folded.

In 1901 Mr. C. Hedley and the writer traced rocks at intervals from a little north of Townsville to near Cooktown, all highly contorted, and all shown on the Queensland geological map as of the same age as the Warwick beds. This caused the writer to consider

[1] "Presidential Address," *Proc. Linn. Soc. N.S. Wales*, 1893, pp. 586–87.

[2] J. E. Carne, "The Tin-Mining Industry of New South Wales," *Mineral Resources No. 14, Dept. Mines, Sydney, N.S.W.*, 1911, pp. 54, 70, 71; E. C. Andrews, "The Drake Copper and Gold Field, N.S. Wales," *Mineral Resources No. 12, Dept. Mines, Sydney, N.S.W.*, 1908, pp. 3–11.

[3] Drake Report. See plate opposite p. 10.

[4] R. L. Jack and R. Etheridge, *Geology of Queensland*, Brisbane (by Authority), 1892, plate 69.

120
125
130
Celebes
Banda Sea
Flores Sea
Arafura
INDIAN
OCEAN
SOUTHERN
WESTERN
AUSTRALIA
Kimberley District
Great Sandy Desert
Great Victoria Desert
Great Australian Bight
Submarine Cables
105
110
115
120
125

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the significance of the peculiar problem of the Permo-Carboniferous, inasmuch as the whole of the work of the field officers of the geological survey of New South Wales had proved that the Permo-Carboniferous strata south of the Hunter River (lat. 33° S.) lie almost horizontally.

The independent testimony of the ore deposits of New Zealand and Australasia was then examined, and the problem of Australasian growth was formulated in the following terms:

During the progress of geological time folding movements in Australasia retreated north and east, while ore deposition moved parallel with these movements.

The growth of New Zealand does not appear to be known definitely, but the New Guinea and the New Caledonian movements appear to have opposed the Australian direction of growth.

A study both of structure and of ore deposits suggests that New Zealand, Australia, and New Guinea have had independent origins.

GROWTH OF AUSTRALASIA FROM PRE-CAMBRIAN TO RECENT TIME

Pre-Cambrian.—The greater portion of Australia, which stretches to the west of a line drawn from the southwest of Tasmania to the center of North Queensland,[1] is composed of pre-Cambrian schists, gneisses, granites, and allied rock types. The dominant strike of the foliations is northwest and southeast, approximately, with a marked tendency to show large local, or even regional, corrugations in the eastern portion of the area. This is well shown on David's map accompanying his report of 1911[2] to the Royal Society. It is possible that at the close of the pre-Cambrian period in Australia the land surface extended across the southeastern or even the eastern portion of the continent. This is suggested, not only by the schists of Cloncurry in Northern Queensland mentioned by Woolnough, but also by the presence of great masses of schists and gneisses of unknown age in Eastern Victoria extending northward into the Cooma district.

[1] W. G. Woolnough, *Bulletin of the Northern Territory*, No. 4, Dept. External Affairs, Melbourne, 1912, p. 51.

[2] "Presidential Address," *Proc. Roy. Soc. N.S. Wales*, 1911. See large map accompanying the paper.

Browne,[1] however, inclines to the belief that a portion of this area, at least, is of Ordovician age. Other schist masses exist in Queensland.

The possibility of sediments and other rock masses being molded onto, or being wrapped round, these resistant blocks is thus suggested.

Cambro-Ordovician.—Since the momentous pre-Cambrian period the greater portion of the area mentioned appears to have been a positive or buoyant element till the present day. A great negative area appears to have existed at this time over Eastern Tasmania, Victoria, and New South Wales. It is possible, however, that a positive element existed in this period in Southeastern Victoria and New South Wales. The Cambrian sediments are more in evidence on the western strip of this area, while the Ordovician are common on the southeastern and eastern portions. It is possible that the Ordovician sediments of the more eastern areas are conformable to the Cambrian, but there is an unconformity between the shallow water forms of the two in the MacDonnell Ranges of Central Australia.

Silurian.—At the close of the Ordovician there was a very powerful folding movement. Wherever the Ordovician occurs in New South Wales or Victoria, it is strongly folded and altered. The new land surface was carried far to the north and east by this folding movement. Ordovician sediments occur quite near the coast about 100 miles south of Sydney, and they outcrop within 120 miles (lat. 33° S.) of Sydney in a direction west-northwest. Thence to the pre-Cambrian outcrops of the more western areas they may be seen in many places, exposed by the stripping of their Devonian cappings.[2] In the majority of the localities observed the strike of the sediments is west of north.

During the Silurian the old negative area which had been occupied by the Cambro-Ordovician sediments once more sank, and the sea transgressed far to the west, almost to Broken Hill (long. 141°

[1] W. R. Browne, "The Geology of the Cooma District, N.S. Wales," *Jour. Proc. Roy. Soc. N.S. Wales*, XLVIII (1914), 172–222.

[2] E. C. Andrews, "The Canbelego Gold and Copper Field," *Mineral Resources, No. 18, Dept. Mines, N.S. Wales*, 1913. See maps and sections.

E.), not nearly so far west, nevertheless, as the Ordovician sea had transgressed. This sea was shallow in places and full of islands. As in the Cambro-Ordovician period, sandy sediments and conglomerates also were deposited in the west, while great areas of coralline limestone were deposited in the eastern portions. Much of the area colored on the geological map as Devonian in the west of New South Wales may be found hereafter to be Silurian or Cambro-Ordovician in age. No fossils have been found in these beds, and they have been referred to the Devonian because of their lithological resemblance to the eastern Devonian quartzites and sandstones. The strikes of the sediments are similar to those of the Ordovician.

Devonian.—A strong movement of folding closed the Silurian and ushered in the Devonian sedimentation. The Devonian problem in Australia is complicated much in the same way as are the Carboniferous, the Permo-Carboniferous, the Trias-Jura, and the Tertiary. The work of the pioneer geologists suggested that there were two, if not three, divisions in the Devonian period, with an unconformity between two of the sets of sediment.

Mr. W. S. Dun has made a study of the Devonian in Australia and he has supplied the following notes for this report. He states that the Buchan and Bindi sediments in Victoria appear to be of Middle Devonian age, and that they are the equivalents, in great measure, of the Murrumbidgee beds in Southern New South Wales, the two groups containing types of fossils in common. In this case, however, Mr. Dun points out that it is probable that after detailed examination, Lower Devonian sediments would be found developed in these regions passing into Middle Devonian.

The Upper Devonian series of sediments are characterized by the forms *Lepidodendron australe*, *Spirifer disjuncta*, and *Rhynchonella pleurodon*. The Upper Devonian series occur both at Mount Lambie and at Tamworth (New England). In the latter locality, however, *Spirifer disjuncta* and *Rhynchonella pleurodon* do not appear to have been found.

Sussmilch,[1] in dealing with the Devonian, says: "An alternative explanation of the relations between the Lower and Upper Devonian

[1] C. A. Sussmilch, *Geology of New South Wales*, 1914, pp. 77–80.

formations, however, suggests itself, and that is that the two formations were deposited more or less contemporaneously, the former in an open but comparatively shallow continental sea, at some distance from a shoreline; the latter in the shallow coastal waters of the same sea" (p. 80). He suggests that the marked differences so well established by R. Etheridge, T. W. E. David, and W. S. Dun between the faunas of the two formations would be due in such case to the unlike environments.

To Benson's field work, however, we are indebted for one definite piece of knowledge which may be expected to help in clearing up the tangle which has gathered round the Devonian in Eastern Australia. He[1] showed that the Carboniferous in New England is actually conformable with the Devonian in that region, the sediments of each age being strongly folded, the strike of the folding being north-northwest approximately, as traced for 200 miles at least.

During various geological surveys in the western, southern, and northern parts of New South Wales, the writer has noted that the Devonian sediments vary in appearance and structure, and the results of those observations would suggest that in very great measure the Devonian sea transgressed the area of folded Silurian sediments as far west as the Darling, without extending, however, as far in that direction as had the Silurian sea. A movement of folding apparently occurred in the Devonian which affected the eastern portions of Southern New South Wales strongly, being more marked as a whole in the northern portion of that area than in the southern, and more marked in the east than in the west. This movement may have been revived still later, with a tendency to cause Australia to grow northward and eastward as it had at the close of both the Silurian and Ordovician periods, the movement of sea transgression to the west and south being less during each succeeding period.

This brings us to a mention of the long zone of weakness extending from a point somewhat south of Sydney to Queensland in a direction slightly west of north. The great negative area which had received the Ordovician and Silurian sediments had been changed to a positive element with the close of the Devonian sedimentation in the south and west. The negative area by this time had shifted

[1] W. N. Benson, *Proc. Linn. Soc. N.S. Wales*, 1913, pp. 490–517.

to the position mentioned above, and such a zone of weakness appears to mark the boundary of two geological provinces in Eastern Australia. Benson has shown that in this heavy area the Devonian and Carboniferous accumulated conformably, none of the series apparently being folded until the close of the Carboniferous. Carboniferous sediments are believed by some geologists to exist in Australia south of this zone, but Mr. W. S. Dun, in a personal communication, has informed the writer that in his opinion the fossils from such sediments are to be referred to the Devonian rather than to the Carboniferous.

It is advisable, at this stage, to consider the general scheme of folding for the Devonian in Eastern Australia, inasmuch as what obtains for the Devonian in a general way, as regards its structure, is true also of the Silurian and the Permo-Carboniferous with this difference, that the analogies of form in rocks of the various periods considered are to be sought in areas which succeed each other to the north-northeast approximately in succession of time. Thus if the Silurian has been folded strongly over a large area, it may be found that the strongest folding of Devonian might be expected to be found north and east of the southern Silurian folds, whereas in certain areas of the strongest Silurian folding the Devonian is to be found bedded almost horizontally.

Thus in Tasmania the Devonian is missing; in Victoria it is folded, apparently in two series separated by an unconformity; in Eastern New South Wales it is strongly folded, whereas in Western New South Wales it occurs as a series of gentle rolls and folds, with small areas, however, exhibiting local nipping or sharp folding within the complex basement of Cambro-Ordovician and Silurian.[1] Reference to forms very similar will be made in the chapter dealing with the Permo-Carboniferous.

It may be mentioned here that a peculiar occurrence of so-called Devonian sediments has been recorded from Northwestern Australia by H. V. Woodward.[2] This observer mentions Devonian

[1] E. C. Andrews, "Canbelego Gold and Copper Field," *Mineral Resources, No. 18, Dept. Mines, N.S. Wales.* See maps and sections.

[2] *Report on Gold Fields of the Kimberley District* (by Authority), Perth, 1891, p. 10. Quoted from T. W. E. David's "Presidential Address," *Proc. Linn. Soc. N.S. Wales*, 1893.

sediments at Kimberley (Northwestern Australia), which are said to possess a strike almost northeast and southwest, and a dip of 70°, while so-called Carboniferous sediments lying immediately above are almost horizontal.

Carboniferous.—Benson's great contribution concerning the conformability of the Carboniferous with the Devonian in Northeastern New South Wales allows us to infer that the Devonian south of lat. 33° S. was folded prior to the tilting of the New England Devonian, and it suggests also that not only Middle Devonian but also Upper Devonian is to be expected in this New England series.

Permo-Carboniferous.—The Permo-Carboniferous period was a most interesting one in Australia, but only the salient points dealing with its history are here recorded so far as they deal with the main thesis of this report.

A great period of submergence is indicated over wide areas throughout peripheral Australia, but the strong folding to which certain sediments of this age were subjected at the close of the sedimentation was confined to a relatively narrow area within Eastern Australia north of the Hunter River, lat. 33° S. Thus the sediments of this age in Tasmania and Western Australia are almost horizontal; in Victoria they appear to be flexed only in the neighborhood of Tertiary faults or monoclinal folds; in Southeastern New South Wales they exhibit corrugations scarcely recognizable;[1] in the coastal region 100 miles north of Sydney they are moderately domed,[2] whereas to the west and southwest they are almost horizontal.[3] In Northeastern New South Wales, as shown by Carne, Woolnough, and the writer, the Permo-Carboniferous sediments are much folded and intruded by granite, whereas at a distance of 200 miles to the west the strata lie almost flat. In Southern Queensland the Permo-Carboniferous is intensely folded, as mentioned elsewhere in this report.

[1] E. C. Andrews, "Yalwal Gold Field," *Mineral Resources No. 9, Dept. Mines, N.S. Wales*, 1901; L. F. Harper, "The Southern Coal Field," *Memoir No. 7, Dept. Mines, N.S. Wales*, 1916.

[2] T. W. E. David, "The Hunter River Coal Measures," *Memoir No. 4, Dept. Mines, N.S. Wales*, 1907.

[3] J. E. Carne, "Western Coal Fields," *Memoir No. 6, Dept. Mines, N.S. Wales*, 1908.

Farther north, as, for example, at Gympie[1] and Mount Morgan,[2] the sediments are strongly folded and are Permo-Carboniferous in age. Along the north coastal area of Queensland there is a very long belt of sediments which are highly contorted and which appear to be Permo-Carboniferous.[3] Nevertheless, less than 100 miles inland the Permo-Carboniferous[4] dips at an angle of 12° only. All this indicates that the close of the Permo-Carboniferous period was accompanied by a strong folding movement in areas north and slightly east of the areas affected by the great closing Carboniferous movement in New England. Additional evidence of this is adduced when dealing with the regions of ore deposition in Australia.

The strikes of these foldings may be considered as subparallel to the Carboniferous lines of Benson, namely, northwest to north-northwest.

It may be pointed out here also that the area of Permo-Carboniferous sediments in Tasmania, South Australia, and Western Australia is very large; nevertheless the beds there are horizontally bedded.

Trias-Jura.—In this period there appears to have been a tendency for the old heavy, or negative, area of Central-Eastern Australia to sag again, or for the long zone of weakness separating New England from the land to the west and south to be broadened. In the northeastern portion of New South Wales and in Southern Queensland another area of sagging received a great thickness of Trias-Jura sediments.

The geographical conditions under which the two sets of sediment were deposited differed in certain well-marked features. This

[1] Jack and Etheridge, *Geology of Queensland* (by Authority), 1892, pp. 72–84. B. Dunstan has also produced detailed geological maps of this area in a late number of the *Queensland Survey Publications*.

[2] Jack and Etheridge, *Geology of Queensland*, p. 598.

[3] Lionel C. Ball, "Wolfram, Molybdenite, and Bismuth at Bamford, Northern Queensland," *Queensland Mining Journal*, 1914, p. 568. Mr. Ball has made a more definite statement as to the Permo-Carboniferous age of the beds in this district in a recent communication to the writer.

[4] J. H. Reid, "Permo-Carboniferous at Bett's Creek," *Queensland Government Mining Journal*, July, 1914, pp. 408–12.

has been indicated most clearly by Carne,[1] who calls attention to the fact that massive conglomerates, coal seams, and abundant fossil trees characterize the northern sediments as to their lower members, while tuffs and sandstones without heavy conglomerates, coal seams, or abundant tree stems characterized the southern and western sedimentation (Triassic). Cross-bedded sandstones of warm-brown color and intercalated black shales characterize the southern, middle, or Hawkesbury series, while cross-bedded sandstones are very common in the northern or Clarence series. In both series the later stages of the Trias-Jura appear to be dark shales in the main.

It is possible that the great folding at the close of the Carboniferous in Northeastern New South Wales was responsible, in great measure, for the heavy conglomerates of the Clarence series as well as of the Triassic of the Upper Hunter valley, and it is probable that very high land barriers separated the two sinking areas during a moderate part, at least, of the period. This might be expected to have caused variations in local floras. As an example, the Clarence series contains a characteristic fossil, namely, *Taeniopteris Daintreei*, whereas it is absent from the Hawkesbury. On the other hand, however, *Taeniopteris Daintreei* is found in the Victoria Trias-Jura, so that Carne and others consider the Clarence to be of different age from that of the Hawkesbury.

The southern or Hawkesbury (Triassic) area does not appear to have been dominated to the west and south by high land, inasmuch as the adjacent and subjacent Permo-Carboniferous in those directions does not appear to have been disturbed except by a gentle movement of sagging. The earlier period of the Trias-Jura appears to have been one of moderate to fair precipitation, but the middle period appears to have been subarid. In the Sydney district massive cross-bedded sandstones predominate in these middle beds, with relatively thin layers of dark-gray shales. In places layers of grit and subangular pebbles are interspersed with large blocks of these dark-gray shales, all mixed confusedly, apparently marking

[1] J. E. Carne, "Western Coal Field," *Memoir No. 6, Geol. Survey, N.S. Wales*, 1907, pp. 26–41; see also E. F. Pittman, *Ann. Report N.S. Wales*, 1880, p. 244. Quoted by Carne, *op. cit.*, p. 26.

periods of short-lived floods (sheet floods) which broke up the clays and mixed them with the pebbles and grits carried downstream by the local cloud-bursts or heavy rains. This appears also to have been the opinion of Professor H. E. Gregory, of Yale, from an examination by him in 1916 of the Sydney and Blue Mountain exposures. The upper portion of the period appears to have been one of greater precipitation in which actual lakes were in existence.

At Sydney, and a little south of that area, the Triassic beds dip inland at a very gentle angle, but, as Carne has shown, the whole southern area of these sediments has a dip averaging only from 1° to 2°. In the northeastern part of New South Wales, however, the Mesozoic coal measures and the conglomerates dip from 10° to 20°, while in Southern Queensland they have been still more disturbed. In the western portions of Eastern Australia, however, as also in Western Australia, they lie practically horizontal.

There appears to be no consensus of opinion among Australian geologists as to the origin of the Hawkesbury beds. Rev. J. E. Tenison Woods considered them to be of wind-blown origin[1] with lakes and swamps between the dunes.

In an unpublished paper R. S. Bonny considers them to be of estuarine origin. On the whole they may be said to be continental in origin, being formed in a sinking area mainly by water strains in a rather dry period.

Cretaceous.—The Cretaceous period marked a spilling over of the ocean with the formation of great epicontinental seas, especially during the Upper Cretaceous period. The area most affected was the northern portion of the old heavy area separating Eastern and Western Australia. It is probable that, during the Upper Cretaceous, the epicontinental sea extended from the Gulf of Carpentaria to the Southern Ocean. The eastern area occupied by the Triassic sediments, however, consisted of dry land during the Cretaceous. At the close of the period the whole center of Australia appears to have been raised to a moderate height above sea-level. Dunstan and Richards have recorded pronounced folding (40°–55°)

[1] "The Hawkesbury Sandstone," *Proc. Jour. Roy. Soc. N.S. Wales*, XVI (1882), 53–90.

of Lower Cretaceous rocks on the coast of Queensland at some distance north of Brisbane.[1]

Reference will be made later to this local evidence of folded Trias-Jura and Cretaceous rocks along the coast of Central and Northern Queensland.

Tertiary period.—The Eocene sea was not large and appears to have been confined to areas, relatively small, in the north and south of the continent. Indeed, the continent as a whole, except in the northeast, appears to have been growing in size subsequently to the close of the Cretaceous, although a submergence, postdating the recent Glacial period, appears to have isolated New Guinea and Tasmania from the mainland.

It is as if there has been a general tendency in Australasia and New Zealand to move in a vertical direction in post-Cretaceous time, the movement being subject to two great laws:

1. That elevation, or vertical movement, of the land was emphasized in an easterly direction, as from Western Australia to New Zealand, due allowance being made for the lagging behind differentially of the two great and relatively heavy portions, namely, Central Australia and the suboceanic mass separating Australia from New Zealand.

2. That the uplifts after the widespread peneplanation of the Cretaceous period did not proceed continuously, but were saltatory in their action, and, moreover, that the periods of time separating these uplifts became less as the present time approached, and that, nevertheless, the amounts of individual uplifts became greater as the periods marking the pauses between the uplifts became less in duration. This has given rise to great "valley-in-valley" structures owing to the interrupted work of the streams.

Thus in Australia, during what appears to be the Cretaceous period, great peneplains were formed in the land areas lying east and west of the Cretaceous sea, and only the hardest rock structures remained to show the existence of former plateaus or hills. In the various Tertiary divisions of time the streams carved valleys with

[1] B. Dunstan, *Queensland Government Mining Journal*, December, 1912; H. C. Richards, "The Cretaceous Rocks of Woody Island," *Queensland Aust. Assoc. Adv. Sci.*, Melbourne meeting, 1913, pp. 719–88.

widths so great as to appear as local peneplains, although they are only very broad, shallow valleys in whose bases other broad and shallow valleys have been excavated. The great uplifts of the later Kosciusko period allowed the streams to form profound canyons which receded along these older shallow valleys. In other words, the main Tertiary land history has consisted of repeated elevations with stream revivals. During one or more of the Tertiary divisions of time, particularly in what may be the Miocene, the land appears to have sunk with the formation of lakelike expanses along the stream courses and the burial, later, of deep-river deposits beneath basalt floods covering thousands of square miles in Eastern Australia. This led to great modifications in the stream drainage, but the dominating lesson of the repeated revival of stream action must not be overlooked, the modifications due to lava floods being only an incident in the great geographical unity of Australia in Tertiary and post-Tertiary times.

New Guinea.—If attention be turned, however, to the northeastern part of Australia, it will be found that as geological time progressed, the area occupied now in part by New Guinea was built to the south and west. An excellent epitome of the main features of structure known to date has been supplied by Professor T. W. E. David.[1] Schists outcrop at very high altitudes along its northern portion, while strongly folded Cretaceous Strata are reported to occur at the highest altitudes in the north, their steep dips ending abruptly against a high and deeply dissected plateau surface. For 50 or 60 miles inland from the south, the area consists of middle and late Tertiary strata, all intensely folded, and all beveled off by a high plain, probably one of submarine erosion. The knowledge of this strong orogenic movement in late or closing Tertiary time and the excavation of a plain of erosion within folded sediments of this age was established by Carne while doing pioneering work in the oil industry.[2]

[1] "Geology of Papua" (Federal Handbook), *Brit. Assoc. Adv. Sci.*, Australian meeting, 1914, pp. 316–25.

[2] J. E. Carne, *Bull. of the Territory of Papua, No. 1*, Dept. External Affairs, Melbourne, 1913, pp. 19–29.

David's conclusion is:

> In regard to the broad tectonic features of Papua it may be suggested, very tentatively, that the mainland of Australia has functioned as a "foreland massif," Torres Straits, the Gulf of Carpentaria, the Arafura Sea, and the deep Mesozoic and Tertiary basins with their thick strata as a *Senkungsfeld*. Possibly the crystalline schists forming a great part of the backbone of the island have played the part of an inner, or *rück-land* massif which has helped to roll up the Mesozoic and Tertiary sediments.[1]

In passing, it may be mentioned that this simply raises the question again as to the origin of the forces of crumpling. Do they act from the land as suggested by Suess in his discussion of the Asiatic framework, or do they act from the oceans? If the movements be assumed to act as from Central Australia toward the oceans, then it is difficult to understand the stability and rigidity of such central area of force. If the source of energy is suboceanic and directed toward the continents, then it is difficult to explain the growth of Australia north and east, while that of New Guinea appears to be south and west, unless, indeed, it be assumed gratuitously that the later foldings in Northeastern Australia are simply the expressions of orogenic movements dying away in a southwesterly direction from the Pacific. Even so the intense contortions evidenced in the Miocene and Pliocene beds might be expected on the northeastern aspect of New Guinea rather than on the south and southwestern. It would seem, indeed, as though each negative or heavy area had played a part in the movements.

New Caledonia.—In New Caledonia the Mesozoic sediments have been intensely folded, especially on the western and southwestern aspects, and the overfolding appears to have been directed toward Australia, according to Peletan, Depiet, Piroutet, and others as quoted by Suess.[2]

New Zealand.—In turning to a consideration of New Zealand we meet with a certain amount of disappointment, inasmuch as there is no consensus of opinion among the workers on certain fundamental points. Thus a glance at Dr. J. W. Gregory's map in the article on New Zealand in the eleventh edition of the *Encyclopaedia*

[1] T. W. E. David, "Geology of Papua," *op. cit.*, pp. 324–25.

[2] *Die Antlitze der Erde* (Eng. tr.), IV, 314–15.

Britannica suggests that this island group was built principally as from southwest to east and north, or at any rate that with the progress of geological time folding movements retreated to the north by east. Marshall, however, in a personal communication, under date of April, 1916, states that much of the New Zealand Jurassic has been confused with the Maitai (so-called Carboniferous) by older workers. Marshall, however, adduces sound reasons for considering New Zealand as being the true boundary of the Pacific Ocean[1] in that portion of its area. Cotton in a recent paper states that the "most profound deformation of this vast sedimentary group [Paleozoic and Mesozoic] took place in Later Jurassic or Early Cretaceous times."[2] He also states that the average trend of the strike of this older mass appears to be west of north (p. 245). And again he writes: "It is apparent that during the period of their deposition [that is, the Tertiary Andes] a great part of the site of the present islands of New Zealand was continually submerged" (p. 247).

Cotton also speaks of orogenic movements in the Pliocene in the northern and more eastern portion of the group, and it is known, moreover, that great volcanic activity has taken place in the northeastern portion of the group with the formation of important gold deposits.

In a personal communication dated August 22, 1916, Cotton writes:

> The early geological history [of New Zealand] is much obscured by the later happenings—a great deal more so, it would appear, than is that of Australia. We cannot even be sure that we have any considerable area of Paleozoic rocks. The small areas of Ordovician and Silurian in northern Nelson we can be certain of, but we know nothing whatever of the relations of these, either to each other or to rocks of later Paleozoic or Mesozoic age. It is the opinion of the present director of the Geological Survey that the greywacke rocks extending southward along the West Coast are of Aorere (Ordovician) age; but they contain no paleontological evidence of age and are part of the "Maitai" system of other writers. As for the "Maitai" rocks throughout

[1] P. Marshall, "Presidential Address," *Geological Section Aust. Assoc. Adv. Sci. Sydney*, XIII (1911), 90–99.

[2] "The Structure and Later Geological History of New Zealand," *Geol. Mag. London*, No. 624, June, 1916.

New Zealand, there seems to be no reason now for classing them as Paleozoic. As regards the Manapouri rocks of southwestern Otago, they may, of course (with the exception of some intrusives), be very ancient; but their relations to other systems are absolutely unknown. It may be that this is an upfaulted block from which a Mesozoic cover has been removed. So far as I know there is no evidence of later formations having been folded against it.

The remarkable flat-lying schists of central and eastern Otago are, again, of indefinite age. Marshall regards them as metamorphosed Mesozoics. He traces a transition to the unaltered "Maitais," but in eastern Otago, along the junction of the schist and greywacke rocks there is a complex of faulted blocks (greywacke now forming the surface in some and schist in others) which had, there can be no doubt, been planed down before the deposition of what I call the "covering strata." Later faults, which affect the cover also, have sometimes followed the lines of the older breaks, *but have reversed the throw*.

As to the direction of folding in New Zealand I have formed no opinion. The latest or Kaikoura folding was accompanied by the formation of great reverse faults in the northeastern part of the South Island, and these hade to the northwest. Many small reverse faults in the Wellington neighbourhood, which intersect (?) Triassic rocks and were *perhaps* developed during the Mesozoic period, hade in the same direction.

One question of great importance is that of the source of the enormously thick "Maitai" sediments, which consist, from end to end of New Zealand, almost universally of the little-worn detritus from acid igneous rocks. Evidently these deposits accumulated not far from a great land mass, but I know of no evidence as to the position of that land mass. Apparently the New Zealand area sometimes formed a part of the continent, for at a number of places there are deposits containing Mesozoic plants.

So far as I know there was no strong folding accompanying the formation of the Hauraki gold deposits, but there have been considerable "block" movements since.

EVIDENCE OF THE ORE DEPOSITION

It is proposed here to see what light may be thrown on the possible structural relations or differences of Australasia, New Guinea, and New Zealand, by a study of the peculiarities of ore deposition in certain areas within these regions. In this connection it is proposed to deal principally with one set of minerals only, namely, the tin group, although conclusions equally interesting would have been forthcoming from a consideration of the gold and copper, together with the silver-lead and zinc groups.

Thus with regard to gold it would have been possible to elaborate with a wealth of detail the knowledge that the gold deposits of

West Australia are found in the great area of pre-Cambrian rocks there developed, and, moreover, that they occur in belts arranged more or less parallel and relatively narrow in width, although in certain localities they appear as small isolated areas or patches; that these narrow and well-defined belts have a general northwest and southeast direction, with divergences in certain instances of several degrees on either side of this direction; that the ore deposits in these belts or zones, owing to certain activities, do not crop out in long and unbroken lines, but are cut up into relatively short lenticles, arranged *en echelon*.

Table I gives the approximate values of the several metals mined in these countries.

TABLE I

APPROXIMATE TOTAL VALUES, IN MILLIONS OF POUNDS STERLING, OF THE MORE IMPORTANT METALS MINED IN AUSTRALASIA AND NEW ZEALAND

	Gold	Copper	Silver-lead	Zinc	Tin	Wolfram	Bismuth	Molybdenite
West Australia..	73.00	11.00	.....	.....	1.00	.....	.....	.....
North Territory.	2.10	0.20	.....	.....	0.33	0.04	.....	.....
South Australia.	1.00	30.00	0.33	.....	.....	.....	.....	.....
Tasmania.....	7.50	11.50	6.50	.....	12.00	0.05	0.02	.....
Victoria.....	300.00	0.25	0.006	.....	0.80	0.005	.....	.....
New South Wales.....	62.00	13.50	74.00	12.00	10.50	0.25	0.166	0.10
Queensland.....	80.00	11.50	2.25	.....	8.50	0.80	0.125	0.25
New Zealand....	85.00	0.02	.....	.....	.....	.....	.....	.....

The general direction of these auriferous belts almost everywhere coincides with the strikes of the schists, which, with one or two exceptions, invariably form the matrices of the gold-bearing reefs. . . . . The quartz reefs are of two distinct types, namely, white quartz reefs and laminated quartz and jasper veins approaching very closely the hæmatite-bearing quartzites which invariably form a conspicuous feature in most of the gold fields of the State. Some of the laminated quartz veins range from almost-pure quartz, through banded jaspers, with crystals of magnetite, to bands appearing to the eye to be virtually pure haematite. The quartz reefs, of what may be called the massive types, occur plentifully in both the schists and the granites.[1]

Like the gold deposits of Western Australia those of the Northern Territory and of South Australia appear to be pre-Cambrian

[1] A. Gibb Maitland, "Mining Fields of Australia" (Federal Handbook), *Brit. Assoc. Adv. Sci.*, Australian meeting, 1914, pp. 447–48.

in age, although there are certain indications of relative youth in the gold deposits of the Northern Territory and South Australia. Eastward, in the old trough lying within New South Wales and Victoria, now filled with Cambro-Ordovician and infolded Silurian sediments, occur the most important gold deposits of Australasia, especially the famous saddle reefs of Bendigo, Ballarat, Canbelego, and other localities. Immediately to the east and north lie the ore deposits beyond the Hunter zone of weakness where Benson's line of serpentine occurs with its gold deposits of Carboniferous age. Beyond, but parallel, or subparallel, with these, are the great gold fields of the closing Paleozoic period in New England and Eastern Queensland, as, for example, at Hillgrove, Gympie, Mount Morgan, and the Palmer. It might be mentioned that, although no gold deposits appear to have been formed in Australia since that momentous period, nevertheless the important gold deposits of the North Island of New Zealand are of *late Tertiary age*. It might be mentioned here that the gold deposits of Southwestern New Zealand appear to occur in Paleozoic rocks.

Or it would have been interesting to enlarge upon the facts connected with the copper deposits of Australia: how in the west they are of pre-Cambrian age, according to Maitland and his geological staff; how the nature of the deposits there suggests deposition at a great depth below the old land surface; how the copper deposits of great but of unknown age, in the Northern Territory, South Australia, Western New South Wales, and Tasmania, as, for example, at Wallaroo, Moonta, Burra Burra, Cobar, Nymagee, and Mount Lyell, do not appear to be dependent upon ordinary igneous rock types, but, from an examination of the reports of Ward, Jack, J. W. Gregory, and the writer, they appear to be the equivalents themselves of igneous rocks because of their peculiar mineral assemblages; how with these famous deposits might be mentioned the great Broken Hill deposit of silver-lead and zinc which is apparently a replacement of schists by garnet, rhodonite, feldspar, and sulphides, owing to the action of vapors arising along a shear zone;[1] how the arrangement *en echelon* of these metalliferous areas and the individual ore lenses within such areas must be sig-

[1] E. C. Andrews, "Broken Hill Lode," *Economic Geology*, October, 1908, pp. 643–45.

nificant in the extreme. It would be instructive also to tell how, in New England, the copper and the gold which were introduced during the Carboniferous folding of Benson occur in the same deposits as a rule, as also do those of the closing Paleozoic both in New England and in the more coastal portions of Eastern Queensland (examples, Drake and Mount Morgan); how also in the Carboniferous of New England the copper and gold depend upon the serpentine belt for their existence, whereas in the Permo-Carboniferous they are related to lamprophyric dykes and basic granitic types.

In New Guinea the copper deposits appear to be in very ancient rocks, whereas in New Zealand copper is practically absent.

The tin group of minerals.—Turning, however, from these interesting points to the tin-wolfram–molybdenite-bismuth group of minerals in Australia, it may be noted that all four may occur together in certain ore deposits in this continent, but as a rule the deposits of commercial importance may be classed under two main heads. Thus tin is frequently associated with wolfram, whereas molybdenite is associated with bismuth. Should molybdenite and bismuth be associated with other minerals of the group, the preference is for wolfram rather than for tin. Indeed, the minerals associated with tin and wolfram, such as tourmaline, topaz, beryl, and quartz with rutile, are practically unknown to the writer in connection with molybdenite deposits.

All these minerals in Australia—tin, wolfram, molybdenite, and bismuth—are associated with siliceous granites or their equivalents. In New South Wales the typical tin-wolfram granites range from 75 to 79 per cent silica, while the typical molybdenite-bismuth types range from 72 to 74 per cent silica. These various granites may be distinguished easily by their peculiar vegetation, and appear to have been the hosts of the tin-molybdenite minerals in Australasia. The vapors which conveyed the minerals of the tin group to the marginal portions of the granites, preferably the roofs, or upper and lateral portions, appear to have varied in their power of penetration. Thus the tin and wolfram deposits, with their boric and fluoric associates, are found in many places at slight distances from the siliceous granites themselves, in rocks such as slate, basic igneous rock, or quartz-porphyry. Always, however,

the tin minerals may be seen to be intimately related to the siliceous granites. The molybdenite deposits are almost always within the marginal development of the siliceous granites, while tourmaline, topaz, and allied minerals are characteristically absent. Contact deposits of molybdenite in Australia, as, for instance, at Yetholme (New South Wales), are rare.

Although these granites in Australia accompanied strong folding movements, and although ore deposits in that continent appear to have been dependent upon strong folding phenomena, nevertheless it must not be inferred that all periods of folding in Australasia have been associated with the formation of ore deposits on a commercial scale, but simply that all ore deposits of commercial importance in Australasia are intimately related in some way to periods of folding. This statement refers, naturally, only to deposits of the metallic minerals.

a) Western Australia: The vast area of Western Australia consists, in the main, of highly altered rocks of pre-Cambrian age. These schists and allied types are intruded by siliceous granites and allied rocks, which also are considered to be pre-Cambrian in age. "The old granite rocks are traversed by many large ice-like quartz reefs. . . . . These older granite rocks . . . . form the matrices of the tin and allied deposits of the state."[1]

This mineral has been found to the extent of about 14,000 tons in Western Australia, while wolfram is subordinate in amount. Molybdenite has been recorded in small scattered flakes from this area.

b) Northern Territory: The rocks of the Northern Territory are extremely old, probably pre-Cambrian in many places. Tin and wolfram to the values respectively of £400,000 and £40,000 approximately have been won from the Northern Territory. Molybdenite has been reported, but it has not been worked as yet.

c) South Australia: The ore deposits of South Australia are very old. Tin, wolfram, and molybdenite have been found in this state, but the amounts won are too negligible to be considered.

[1] A. Gibb Maitland, "Mining Fields of Australia" (Federal Handbook), *Brit. Assoc. Adv. Sci.*, Australia meeting, 1914, pp. 446–47.

d) Tasmania: The tin, wolfram, molybdenite, and bismuth deposits of Tasmania are considered to be of closing Silurian or early Devonian age.[1]

The tin production exceeds £12,000,000 and the wolfram £50,000 in value. Molybdenite has not been worked, but bismuth to the extent of about £200,000 value has been won.

e) Victoria and Southeastern New South Wales: In Victoria the age of the tin, wolfram, molybdenite, and bismuth deposits is not known definitely. The value of the tin won is slightly less than £1,000,000, that of the wolfram about £5,000, while molybdenite and bismuth have been found only in very small quantities.

Probably Victoria and Southeastern New South Wales form one geological province, and in the latter area the tin and allied minerals may be considered as of post-Devonian and of pre-Permo-Carboniferous age. Tin is relatively rare, but molybdenite and bismuth are abundantly represented.

f) Northeastern New South Wales and Eastern Queensland: The northeastern portion of New South Wales appears to be a province geologically distinct from that of the southeastern portion of the state, and the tin, wolfram, molybdenite, and bismuth deposits found there appear to be closing Paleozoic in age. These deposits are confined to a strip less than 150 miles from the coast. The commercial molybdenite and the bismuth occur within the eastern zone, while the commercial tin occurs within the western zone. The small deposits of the far west, near Broken Hill, for example, apparently are of very early Paleozoic age, and they really belong to the South Australian region or province.

The value of the tin won from New South Wales exceeds £10,500,000, the wolfram values approximate £200,000, the bismuth £150,000, and the molybdenite about £100,000.

In this connection it should be remembered that until 1902 molybdenite was considered as an impurity in the bismuth, its

[1] W. H. Twelvetrees, "The Scamander Mineral District," *Bull. No. 9, Geol. Survey, Tasmania*, 1911, p. 23–24; other official reports of great interest dealing with the subject of mineral deposits in Tasmania are by L. K. Ward, Loftus Hills, and L. L. Waterhouse.

inseparable associate in New South Wales; thus great amounts of the molybdenite have been lost.[1]

Queensland, in its eastern portion, should be considered as belonging, probably, to the same geological province as New England, or Northeastern New South Wales.

The tin, wolfram, molybdenite, and bismuth deposits are found only within the eastern strip of the state, and their age appears to be the close of the Permo-Carboniferous. The granites and mineral associations of the two areas are almost identical also. Thus this great province of Eastern Queensland and New England, which has yielded the bulk of the world's supply of molybdenite, lies on a great flat arc having a general trend of northwest to north-northwest. These ore deposits are associated with strong movements of folding, the age of which appears to be closing Paleozoic.

The approximate values of the tin, wolfram, molybdenite, and bismuth won from Queensland are respectively £9,000,000 to £10,000,000, £1,000,000, £250,000, and £150,000.

It would thus appear that the deposits of the tin and molybdenite group of minerals in the great geological province of Western Australia, South Australia, and the Northern Territory are of great age, but that they are almost negligible in commercial value. It is not known, however, what proportion of this absence is due to removal by erosion of the upper portions of the granites. The deposits of this group in Tasmania may be of closing Silurian or early Devonian age, the tin values being very large, but wolfram, molybdenite, and bismuth are unimportant; the deposits of the geological province of Southeastern Victoria and Southeastern New South Wales are important and are post-Devonian and pre-Permo-Carboniferous in age; while the deposits of the province of New England and Eastern Queensland, forming a coastal fringe to Northeastern Australia, are highly important from a commercial point of view and appear to be closing Paleozoic in age.

[1] Official reports have been written on the tin and molybdenite areas of New South Wales by T. W. E. David, J. E. Carne, and the writer, while Professor Leo A. Cotton has published reports on the tin of New England in the *Proc. Linn. Soc. N.S. Wales* XXXIV (1909), 738–81; Cotton intends to continue the study of tin genesis in Australia in the near future.

All of these Australasian deposits are intimately related to strong movements of folding, accompanied by intrusions of very siliceous granite.

No molybdenite, bismuth, wolfram, nor tin of any commercial importance whatever appears to have been found in New Guinea, New Caledonia, or New Zealand, although molybdenite and allied minerals have been recorded as curiosities in older Paleozoic granites in New Zealand. Neither are there in New Zealand any important copper deposits similar to those which are so intimately associated with the gold, tin, and molybdenite in Australasia.

CONCLUSION

It is therefore permissible, perhaps, to infer that each of the three great groups, namely Australia, New Guinea, and New Zealand, is a distinct geological province, but whereas in New Guinea the movements appear to have opposed the Australian growth with a tendency to fill the intervening negative area; on the other hand the growth of Australasia and New Zealand appears to have been intimately related in some manner, as though each had grown sympathetically in response to some simultaneous dominating agency. The folding action ceased in the Australasian area long before it did so in the New Zealand area. The foldings in New Guinea also were maintained right into recent geological time.

Here again the ore deposits proclaim the independence of the three centers. The oil fields of New Guinea suggest the Burmese or Malaysian origin of the New Guinea lines of structure,[1] and in a similar way the tin-wolfram–molybdenite-bismuth group of minerals appears to mark the real limits of Australasia. A little of the molybdenite group occurs in the New Zealand area, in the very old rocks, but the group as a whole, with its grand suite of siliceous granite horsts, may be said to end at the east side of Australasia. Moreover, as the folding movements retreated east and west, with progress of time they appear to have passed away finally to the northeast from Southwestern Australia toward New Caledonia.

[1] T. W. E. David, "Geology of Papua" (Federal Handbook), *Brit. Assoc. Adv. Sci. Australia*, 1914, p. 320.

It is therefore permissible, perhaps, to infer that the Tasman Sea is of great age, especially in its more southern portions, inasmuch as it appears to have been a barrier to common or related ore deposition between Australasia and New Zealand through the ages.

This of course does not imply that Australasia and New Zealand have not been closer together in the past, nor that Australasia has not extended considerably farther to the east in former times, especially in its northeastern portions; it simply suggests that some great agency which controlled the growth of Australasia and New Zealand appears to have admitted a negative or relatively sunken area from early times in the region of the Tasman Sea, and that this agency had faded away to epeirogenic movements in the Australasian area while yet it was vigorously folding the New Zealand rocks.

All this appears to be in harmony with the general contention of Marshall[1] who maintains that New Zealand, and not Australia, lies on the real border of the Pacific. Marshall, however, approaches the subject from a point of view entirely different from that taken in the present note.

[1] P. Marshall, "Presidential Address," *Geological Section, Australian Assoc. Adv. Sci. Sydney*, XIII, (1911), 90–99.

A BOTANICAL CRITERION OF THE ANTIQUITY OF THE ANGIOSPERMS

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As to the origin of the angiosperms, that group of seed plants which is now such a dominant element in the earth's vegetation, we know almost nothing. They first appear as fossils in the deposits of the lowest Cretaceous in eastern North America, Alaska, Greenland, and Portugal, but just where they actually originated, and how long ago, are still matters of great uncertainty. The aim of the present paper is to throw a little light on the antiquity of this great plant group by studying the rate of evolution displayed by its members.

Evolution has not been a uniformly rapid process. The fact that plants recognized as "primitive" and others recognized as "recent" exist together at the present time makes it evident that certain vegetable types have changed but little throughout long geological periods, whereas others have for one cause or another become altered much faster. The degree of inherent "variability" and the frequency of hybridization have doubtless been influential in determining this rate of change, but a more important factor perhaps than either seems to be the length of the generation or period from seed to seed. A plant in which this cycle is completed in a year or two is able to multiply its generations more rapidly, and thus to accumulate heritable changes much faster, than one which requires a longer time for the attainment of reproductive maturity. This length of generation is definitely correlated with the growth habit of the plant, being greatest in trees—which usually reach an age of from fifteen to twenty years (in many cases much more) before bearing fruit—less in shrubs, and shortest of all in herbs, where one or two seasons from seed suffice to produce a fruiting plant again. In a given length of time, therefore,

a herbaceous species will pass through a much larger number of generations than a woody one, and will consequently tend, other factors being equal, to become changed in type much more rapidly. We should thus expect the herbaceous element in the vegetation to have been evolved at a much faster rate than the woody element. The establishment of this as a fact, taken with what we know as to the history and present numerical status of herbs and woody plants, will provide us with a valuable clue as to the antiquity of the angiosperms.

That herbs are indeed subject to more rapid changes than any other plant type is indicated by the fact that the first local species and genera to develop in a region subsequent to its isolation have apparently almost always been herbs. This is well illustrated by a comparison of the floras of temperate North America and of Europe. On these continents today there are many local or "endemic" genera which are limited in their distribution to one or to the other. Certain of these are evidently "relict" endemics, isolated survivors of types once much more widely disseminated. They may be recognized from the fact that they stand without near relatives in the floras; and many of them, such as sassafras and hickory, occur as fossils on both sides of the Atlantic. These relicts doubtless constitute a very ancient floral element, and it is significant that among them are practically all the genera of trees and shrubs which are local to either North America or Europe. The majority of the endemic genera, however, seem to belong to quite a different category, for they occur in groups of from three to twenty genera, the members in each of which are closely related to one another, each group apparently to be looked upon as a separate center of evolution and the nucleus of a new family. The genera centering around *Lesquerella* in the Cruciferae, around *Eriogonum* in the Polygonaceae, around *Godetia* in the Onagraceae, around *Pentstemon* in the Scrophulariaceae, and around *Solidago* in the Compositae, are a few of the sixty or more such groups in the dicotyledonous flora of North America, and there are as many in Europe. These "indigenous" endemic genera most probably had their origin on their respective continents, since a free interchange of plants between America and Europe was interrupted, presumably

in the Early or Middle Tertiary; for had they existed before that date in anything like their present numbers and importance, it is highly unlikely that they would now be represented in the floras of both hemispheres. During the time since the isolation of the two continents, and while the rest of the flora have remained unchanged or have been developing endemic species merely, these plants have evidently undergone much wider changes, until they have finally given rise to new generic types. We are thus forced to conclude that the indigenous endemic genera constitute the most rapidly evolving members of their flora; and it is significant that they include practically nothing but herbaceous species—surely excellent evidence that the herb changes in type more rapidly than the tree or the shrub.

Further evidence pointing to the same conclusion is presented by a study of the distribution of herbs and of woody plants in the modern scheme of botanical classification, for herbs are found to occur in larger groups than woody plants, their genera containing more species and their families more genera. Monotypes and very small genera and families are very much less common among herbs than among woody plants. These facts are what one might expect on the supposition that herbs are changing faster than the rest of the angiospermous vegetation, for the more rapid production of new forms leads to the building up of larger aggregations, and enables genera or families which have become reduced in size through extinction to repair these ravages quickly.

A study of the structure, distribution, and ancestry of herbaceous angiosperms[1] indicates that they have been evolved in comparatively recent times from a woody ancestry, and have undergone practically their whole course of development since the beginning of the Tertiary. As opposed to this rapid change among herbs, we know from fossil evidence that very many woody genera have existed with very little alteration for a much more extended period than the length of the Tertiary—a convincing demonstration of the slowness with which trees and shrubs undergo evolutionary change. Almost all our woody genera bear evidence, in present

[1] E. W. Sinnott and I. W. Bailey, "The Origin and Dispersal of Herbaceous Angiosperms," *Annals of Botany*, XXVIII (1914), 547–600.

distribution or fossil remains, of a considerable degree of antiquity.

To corroborate this testimony as to the relative rapidity of evolution in herbs and in woody plants, data as to their actual rate of change today would be highly desirable; but this is very difficult to obtain. As far as differences in "variability," using the term in its broadest sense, are concerned, the two growth forms seem nearly equal. In both there are many highly variable types and many of great constancy. In the floras of three representative regions—Eastern North America, Australia, and Ceylon—the proportion of varieties and named forms among the woody species is found to be practically the same as among herbs. Nor is there a radical difference between the two in the extent of cross-pollination by insects, although in temperate regions this is somewhat more common among herbs than among trees and shrubs. The difference in length of generation to which we have called attention is probably the most important factor in determining the rate at which they have evolved.

To whatever cause we may attribute it, however, there seems to be little doubt that during the evolution of the angiosperms the primitive, woody element has been developed very much more slowly than the more recent, herbaceous one; and it is this difference which gives us a hint as to the antiquity of the whole group. We find in the angiosperm flora today (dicotyledons alone considered) over 4,200 genera of trees or shrubs, as opposed to only 2,600 genera of herbs. We may be reasonably sure that practically all of these 2,600 genera of herbs have been developed since the beginning of the Tertiary; and if we assume that herbs are producing new types only twice as fast as trees and shrubs—surely a conservative estimate—we must believe that only about 1,300 woody genera have been evolved during the same time. The evolution of the 4,200 genera of woody plants at present existing, to say nothing of the great numbers which have been lost through extinction (by which trees and shrubs have suffered much more than herbs), would therefore require a period at least thrice the length of the Tertiary. If the common assumption that the Tertiary was approximately as long as the Cretaceous is correct,

the origin of the angiosperms would thus be thrust back to a date much earlier than the beginning of the Cretaceous.

Of course such an estimate is hypothetical in the extreme; but by indicating that the history of the woody members of the group extends back over a period many times as long as that during which herbs have existed, it serves to give us a clue as to angiosperm antiquity, and it emphasizes the fact that our present huge array of trees and shrubs, types very slow in changing, must have required an enormous length of time for their evolution. There is evidence, moreover, that evolution took place even more slowly in former times than it does at present, since flower-loving insects, to the agency of which many attribute the rapid development of the angiosperms, did not appear on the scene, at least in numbers, till the dawn of the Tertiary.[1] All this makes it highly probable that these now dominant seed plants did not begin their existence in the early Cretaceous, where they first appear as fossils, but that they had already undergone a long course of evolution before that time. Indeed, the external features, and more particularly the internal anatomy, of these earliest fossil angiosperms are not at all those of primitive types, but exhibit a considerable degree of specialization.[2] To regard such plants as having sprung suddenly into being from gymnospermous ancestors is to overtax the imagination of even an ultra-mutationist.

As to why the earliest members of the group apparently failed to be preserved we cannot be sure, but evidence is at hand that they were upland forms which would tend less frequently to become fossilized. This predilection of primitive angiosperms for an equable, reasonably cool climate, if it can be proved, will lead us to look back to the era of low temperatures in the Jurassic, or perhaps even to as remote a period as the cataclysmic refrigeration of the Permian, for the date when the first angiospermous stock began to be differentiated from its gymnospermous ancestry.

The botanical evidence is therefore overwhelmingly in favor of the conclusion that angiosperms existed for a considerable period

[1] Handlirsch, *Die fossile Insecten*.

[2] M. C. Stopes, "Petrifactions of the Earliest European Angiosperms," *Phil. Trans. Royal Society*, B, 203, pp. 75–100.

previous to the Cretaceous, although this cannot be said to be absolutely proved till they are brought to light as fossils from the earlier periods of the Mesozoic, a discovery which diligent search may reasonably be expected to yield. The establishment for the angiosperms of an antiquity greater than that usually accorded them at the present time will be of some importance geologically, since the occurrence of fossil members of the group in a given formation will no longer be regarded as a demonstration of the post-Jurassic age of the latter.

ARE THE "BATHOLITHS" OF THE HALIBURTON-BANCROFT AREA, ONTARIO, CORRECTLY NAMED?

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The large areas, composed essentially of banded red gneiss, which are found throughout the Haliburton-Bancroft area have been called by Adams and Barlow "batholiths."[1] These appear on maps of this region as circular or oval masses more or less completely surrounded by sediments or schists of sedimentary origin. The stratification of these sediments follows in strike the boundaries of the adjacent gneiss. Moreover, within the gneissic areas are layers of amphibolite or gray gneiss which conform in dip and strike to this same boundary. The gneissic areas, therefore, may be described as domes of red granite gneiss containing gray gneiss and amphibolite in layers striking concentrically to points more or less fixed within the mass and dipping quaquaversally at angles which vary from 37° to 45°.

In his earlier writings, F. D. Adams stated three views as to the origin and method of emplacement of the "Fundamental Gneiss."

1. The Fundamental Gneiss may be the remains of a primitive crust which was penetrated by great masses of igneous rocks and subjected to successive dynamic movements. The Grenville series may be an upward continuation of the Fundamental Gneiss under altered conditions, marking a transition from a primitive crust to normal sediments.

2. The Grenville series may be considered as distinct from the Fundamental Gneiss and reposing on it unconformably, being a highly altered series of clastic origin; the Fundamental Gneiss having some such origin as suggested above or being an older intrusive series of still more highly altered sediments.

3. The fundamental Gneiss may be considered as a great mass of eruptive rock which has eaten upward and penetrated the Grenville series, while the

[1] *Geol. Surv. Can., Mono.*, VI (1910), 12.

Grenville series represents a series of altered sediments of Laurentian, Huronian, or subsequent age.[1]

Adams in the same article stated that the last hypothesis was untenable.

The world-wide distribution of the Fundamental Gneiss (forming, as it does, wherever the base of the geological column is exposed to view, the foundation upon which all subsequent rocks are seen to rest) is opposed to this view as is also its persistent gneissic or banded character.[2]

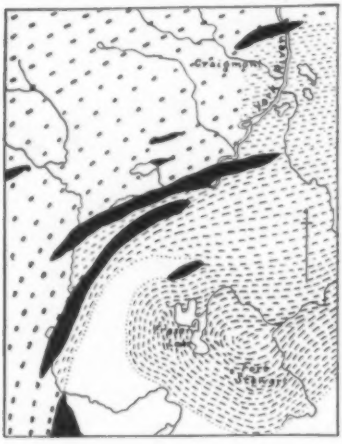

FIG. 1.—Map of the corundum syenite district of Craigmont, Ontario.

Black, limestone; white, amphibolite; dashed, gneissic granite; dotted, gneissic granite with amphibolitic inclusion.

Later, in 1897, Adams altered his earlier view. He writes:

The batholiths are undoubtedly formed by an uprising of the granitic magma from below, and these foci indicate the axes of greatest upward movement. These centers are not all areas of most rapid uplift, however. On the contrary, the gneissic foliation in some cases dips inward in all directions toward the center, thus marking them as places where the uprise of the magma was impeded, that is to say, places where the overlying strata have sagged down into the granite magma.[3]

A striking fact concerning these so-called batholiths is that they do not cut across the structure of the invaded rocks, a fundamental characteristic by which post-Cambrian batholiths are recognized. While it is true that there are bodies within the district which cut across the structure of the country rock, they are unusual, and concordant relationships are much more common.

A glance at the map (Fig. 1) which shows the corundum syenite district of Craigmont, Ontario, makes clear the concentric arrangement of the sedimentary rocks within the gneiss areas.

[1] *Journal of Geology*, I (1893), 330–32.

[2] *Op. cit.*, 332.

[3] *Am. Jour. Sci.*, III (1897), 173–80.

The granite was intruded between the layers of limestone. As in the present case, long narrow layers of limestone are often found isolated in the gneiss. These layers are in parallel bands and the strike of their stratification conforms to the strike of the gneissic structure of the surrounding granite.

If the boundary between gneiss and pure limestone is sought, it will invariably be found that there is a transitional contact zone. The distinction between areas which may be designated as "Gneiss with amphibolitic inclusions" or "Amphibolite" or "Limestone invaded by much gneiss" depends upon the degree to which the granite has invaded the limestone and altered it to amphibolite. In general, on crossing the strike from limestone to amphibolite, there is a gradual transformation of one rock into the other. The amphibolite in turn is transitional to red gneiss through the intermediate stage of gray gneiss. Xenoliths of amphibolite within the gneiss are in no degree so abundant as stringers of amphibolite varying from a few centimeters to a meter in diameter and the schistose structure of which conforms to the gneissic structure of the granite and the stratification of the limestone.

Adams[1] attributes the parallel arrangement of these bodies to movements of the granite after intrusion. He conceives that the limestone blocks, stoped from the roof of the batholith, were softened by heat and pulled out into lenses by flowage.

The parallel banding of pre-Cambrian rocks is not a local feature, illustrated only in the rocks of the Haliburton-Bancroft area. It is, rather, characteristic of most pre-Cambrian terranes. The interbanding of gneiss of igneous origin with sediments is shown by Lawson in his study of the Lake of the Woods. Högbom[2] has described similar relationships which are shown by the rocks about Upsala, Sweden.

The gneiss of the pre-Cambrian of the Adirondacks is so mingled with limestone and other sediments that for years it has been a mooted question whether to consider it of igneous or sedimentary origin. It forms lenses and sheets in the sediments, or traverses them so irregularly that an exact interpretation is difficult.

[1] *Can. Geol. Surv., Memoir No. 6* (1910), 73–78.

[2] *Bull. Geol. Instn., Univ. Upsala*, X (1910–11), 39.

However, C. H. Smyth, Jr.[1] and H. P. Cushing[2] now consider them igneous.

The gneisses of the Highlands of New Jersey may be described in similar language. These are considered by W. S. Bayley[3] and C. N. Fenner[4] to be sediments invaded by granite.

G. M. Dawson writes as follows concerning the Shuswap Terrane of British Columbia:

> The Shuswap rocks proper evidently represent highly metamorphosed sediments with perhaps the addition of contemporaneous bedded volcanic materials. . . . . These bedded materials are, however, associated with a much greater volume of mica-schists and gneisses of more massive appearance, most of which are evidently foliated plutonic rocks, and are often found to pass into unfoliate granites. The association of these different classes of rocks is so close that it may never be possible to separate them on the map over any considerable area. . . . .
>
> A distinct tendency to parallelism of the strata or foliation with adjacent borders of the Cambrian system has been noted in a number of cases. This might imply that the foliation was largely produced at a time later than the Cambrian, but materials of some of the Cambrian rocks show that the Shuswap series must have fully assumed their crystalline character before the Cambrian period. *It seems, therefore, probable that the foliation of the Shuswap rocks may have been produced rather beneath the mere weight of superincumbent strata than by pressure of a tangentical character accompanied by folding.*[5]

R. A. Daly, in a recent report on this same series, states that it has been injected by innumerable sills and laccoliths. He concludes:

> The extraordinary prevalence of sills and other concordant injections is explained by the extreme fissility of the Shuswap sediments and greenstones. This feature is due to static metamorphism.[6]

Two hypotheses are offered, therefore, to explain the parallel banding of pre-Cambrian rocks. In the Haliburton-Bancroft area, Adams conceives that, in the process of intrusion by magmatic

[1] *N.Y. State Mus., 41st Ann. Rept.*, II (1899), 469–97.

[2] *Bull. No. 115*, N.Y. State Mus. (1907), 451–531.

[3] *U.S.G.S., Raritan Folio*, No. 191.

[4] *Journal of Geology*, XXII (1914), 594 ff.

[5] *Bull. Geol. Soc. Am.*, XII (1901), 63–64.

[6] Ann. Rept. Dept. Mines, *Can. Geol. Surv.* (1911), 3–12.

stoping, blocks of limestone were torn from the roof of the invading granite batholiths and elongated parallel to the contact of the granite and limestone by movements of the granite as it consolidated.

Daly believes that static metamorphism produced planes of weakness within the Shuswap series and that sills of granite were intruded along these planes.

Fenner,[1] discussing the method of intrusion of the granites of the New Jersey Highlands, states that in his opinion gaseous emanations from the granite magma penetrated the sedimentary rocks along planes of weakness and prepared the way for the intrusion of granitic fluids. The intrusion of these fluids produced banded gneisses.

It is, of course, entirely possible that intrusion in the Haliburton-Bancroft region took place by magmatic stoping and that this region is not analogous to the others described. The rock types vary within the several areas and there is, necessarily, a corresponding change in the structural relations. The limestones of the Grenville series would undoubtedly be more altered by the metamorphic effects of the granite than the quartzose rocks of the New Jersey Highlands and the Shuswap series.

It seems to the writer, however, that the facts shown by the study of the Glamorgan gneissic area favor the theory of intrusion by parallel penetration along planes of weakness rather than the theory of intrusion by magmatic stoping.

If intrusion took place by magmatic stoping, the following conditions must be postulated. The objections to each of these conditions are noted.

1. The blocks from the roof of the batholith were stoped off and elongated parallel to the contact.

1. Intrusion by magmatic stoping usually produces an irregular molar contact. The igneous rock cuts the sediments. Though the blocks were elongated parallel to the contact, this would not, except by chance, be parallel to the stratification of the sediments, and yet this is the relationship of the banded structure of the rocks of the Haliburton-Bancroft area.

[1] *Journal of Geology*, XXII (1914), 594 ff.

2. Adams postulates that the elongation of the blocks occurred in the later stages of batholithic intrusion as the granite solidified.

2. It would seem necessary that this should be true; for a hot, fluid magma, if too hot would melt the blocks and incorporate them into a homogeneous magma. If it were too cold it could not elongate them. The necessary conditions for the production of parallel elongation, therefore, is a narrow temperature range within which the blocks remain viscous. This would be found, it would seem, at a more or less constant distance from the molar contact of the intruding batholith with the country rock. As the magma progresses upward, the central heat of the batholith must likewise progress upward and hence the parallel banding of the batholith produced at any stage would be destroyed in a later stage by the complete solution of the blocks into a homogeneous magma. The so-called "batholiths" are in all stages of dissection yet the parallel structure is persistent from center to edge. The structure is not, therefore, a border phenomenon as Adams' theory would demand.

3. The limestone blocks, stoped from the roof of the granite batholith, floated and so were elongated by the movements of the granite parallel to its contact.

3. Daly[1] has shown that limestone blocks at high temperatures are much heavier than fluid granite. Hence these blocks should sink and leave a clear contact which would be progressively attacked by the hot granite magma. This would not give rise to the parallel structure observed. If, however, they floated they would impede the attack of the granite magma at the contact and their solution or partial solution and elongation would cause an enormous loss of heat and render the further upward progress of the batholith very difficult.

It is estimated by Adams and Barlow that 20 per cent of the "batholithic" areas consist of gray gneiss and amphibolite. This estimate is low for the districts visited by the writer. Not only these rocks but also bands of pure limestone are often found near

[1] *Igneous Rocks and Their Origin* (1914), 202.

the center of the gneissic areas, where, it would seem, by Adams' theory, that the pure gneiss of the intruding batholith should be found.

This fact makes it easy to believe that the granite was intruded along planes of slight resistance, and that the limestone terrane of the Grenville series became an immense steam pack, at the time of the intrusion of the granite, with layers of gases followed by fluid granite alternating with layers of limestone. The pre-Cambrian granites were probably accompanied by an immense amount of pneumatolytic gases. The loss of these gases at higher levels due to decreasing pressures accounts for the gradual lessening of the interaction of granite and limestone away from the main granite mass, while within the gneissic areas the retention of the gases allowed the granite to effect a complete change of the limestone to gray gneiss and amphibolite. At certain places the granite failed to penetrate great lenses of the limestone. The gases from layers of fluid magma at the top and bottom of these lenses metamorphosed their borders but failed to affect their centers. Lenses of pure limestone were, therefore, preserved in the midst of gray gneiss and amphibolite.

It has been inferred that the intrusion of the granite occurred along planes of weakness. These, as the structure now shows, were parallel to the stratification of the limestones. Daly and Dawson have stated that in the Shuswap area these planes were due to static metamorphism. Fissility produced in this way would be less apparent in limestones than in the quartzose rocks of British Columbia. However, the fact that the gneissic structure of the Laurentian gneiss is parallel to the stratification of the Grenville series would favor the view that the granite solidified under conditions of stress similar to those which produced the parting planes along which it was intruded. A vertical dike near Baptiste Lake, west of Bancroft, Ontario, shows horizontal schistose structure similar to certain dikes described by Daly[1] in the Shuswap area. Adams and Barlow[2] ascribed the gneissic structure of the Laurentian gneisses to the pressure of intrusion of the granite magma. This,

[1] Cf. to figure in *Guide Book No. 8*, Part II, Internat. Geol. Cong., Can (1913), 130.

[2] *Can. Geol. Surv., Memoir No. 6* (1910), 78–81.

however, would not explain a phenomenon such as that shown by the dike just described (Fig. 2). It is believed that the fissility which allowed the granite to intrude the sediments and the gneissic structure of these granites were both results of a persistent force, the static pressure of the overlying sediments. The Grenville series is said by Adams and Barlow to be approximately 50,000 feet thick. This series compares with the Shuswap Terrane which is 30,000 feet thick.

The elongation or compression of the amphibolitic layers and the presence of amphibolitic inclusions may be explained as easily by the theory of Daly and Fenner as by that of Adams. The granitic gases and fluids must have had their origin at certain definite points. At these points they were pushed upward and sideways along planes of easy parting and a pine-tree structure was produced. In general, the increase of material due to the addition of granite would produce a doming at the center of intrusion with quaquaversal dips away from these points. However, the subsidence of the magma on cooling might very possibly cause a collapse of the dome and irregular dips would result.

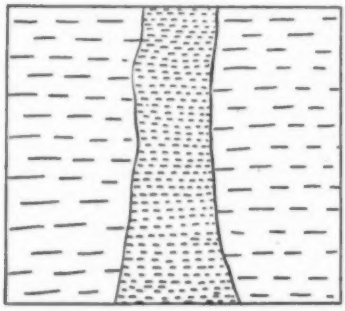

FIG. 2.—A vertical dike showing horizontal schistose structure.

The mechanism of lit-par-lit intrusion, as explained by Fenner,[1] is dependent on the fluxing power of the pneumatolytic gases given off by the granite. These go before and prepare the way for the later intrusion of the granite magma. The prevalence of lit-par-lit rather than batholithic intrusions in pre-Cambrian terranes may be due, therefore, to the greater abundance of magmatic gases in the earlier periods of the earth's history. The vast amounts of pegmatitic granite associated with pre-Cambrian areas lends support to this theory.

[1] *Journal of Geology*, XXII (1914), pp. 594 ff.

The facts presented above do not mean that cross-cutting bodies are lacking in the Haliburton-Bancroft areas. They are found but are by no means as common as concordant injections.

It seems fair to conclude, therefore: (1) that the so-called "batholiths" were formed by the concordant injection of granite into a fissile limestone terrane; (2) that this fissility was produced by the pressure of the overlying sediments; (3) that the layers of

FIG. 3. "Stromatolithic" structure (C. N. Fenner, *Jour. Geol.*, XXII [1914], p. 596).

limestones, lying between layers of molten granite, were permeated by the pneumatolytic gases and fluids given off by the granite and transformed to amphibolites or gray gneisses; (4) that the concordant injection of the granite produced the dome-like character of the "gneissic" areas; (5) that the term "batholithic" does not describe the true character of these areas and the term "stromatolithic"[1] is suggested in its place (Fig. 3).

[1] From Greek στρωμα, "a layer," and λιθος, "a stone." The noun "Stromatolith" may be defined as a rock mass consisting of many alternating layers of igneous and sedimentary rocks in sill relationship.

A CONTRIBUTION TO THE OÖLITE PROBLEM

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INTRODUCTION

At the present time there are two prevalent theories of oölite formation, namely, the inorganic, or chemical precipitation theory, and the organic theory. Prior to the year 1890 the inorganic theory was generally agreed to and it is to this day the most widely accepted of the two.

In the year mentioned, however, Wethered[1] pointed out a close relationship between the concretionary structure of the calcareous algae *Girvanella* and that of true oölite, and showed that certain so-called oölites of the Carboniferous and Jurassic of England really consist, in part at least, of rounded calcareous masses secreted by this organism, since they possess in addition to the concretionary structure the vermiform tubules which characterize the genus. But in this and again in a succeeding paper, entitled "The Formation of Oölite," which appeared in 1895,[2] Wethered was unable to demonstrate the presence of the *Girvanella* tubules in typical oölite spherules showing both radial and concentric structure, although he was led to believe that these were also of algal origin.

Following closely upon Wethered as a champion of the organic theory came Rothpletz, who published a paper on the origin of oölite in 1892.[3] This investigator upon studying the recent oölites of Great Salt Lake found that where these were still in the water they were usually covered by a bluish-green algal mass consisting of the cells of *Gloeocapsa* and *Gloeothece*, forms which are known to secrete carbonate of lime; and, when the oölite grains and rodlike

[1] *Quar. Jour. Geol. Soc. London*, XLVI, 270–83.

[2] *Ibid.*, LI, 196–209.

[3] *Botanisches Centralblatt*, No. 35, pp. 265–68 (English translation by F. W. Cragin, *American Geologist*, X, 279–82).

calcareous bodies on the shore were dissolved in acid, they all yielded dead and shriveled fission algae. Rothpletz, therefore, concluded that the oölites of Great Salt Lake are the product of lime-secreting fission algae, and that their formation is proceeding day by day.

Furthermore, a study of the recent and near-recent oölites of the Red Sea showed these also to contain minute grains of organic material suggesting fission algae. But these differ from the Great Salt Lake oölites in that their nuclei always consist of sand grains and in that their concentric structure is less well developed. They also possess small vermiform canals filled with calcite, which are interpreted as imprisoned algae of another type.

Rothpletz also remarks that certain elongated corpuscles possessing oölitic structures, which he interprets as organic, occurs in the Lias limestone of the Vilser Alps, and concludes as follows: "According to the present stage of my researches, I am inclined to believe that at least the majority of the marine calcareous oölites with regular zonal and radial structure are of plant origin; the product of microscopically small algae of very low rank, capable of secreting lime."

In spite of these discoveries by Wethered and Rothpletz, later students of the oölite problem have tended to drift back to the inorganic theory and to regard the association of oölites with algae as accidental. Thus Linck[1] has shown by experiment that oölites similar to natural ones may be produced artificially by the action of sodium carbonate and ammonium carbonate on the calcium sulphate of sea-water. He points out that these carbonates are formed by decomposition of animal and plant tissues in the sea, and favors the view that oölites have been formed in this way. That natural oölites can be formed chemically is demonstrated by Vaughan,[2] who points out that oölitic structure is now being developed in the calcareous muds precipitated through the agency of bacteria off the coasts of Florida and the Bahamas.

In a recent review of the whole question of oölite formation, T. C. Brown[3] has endeavored to substantiate Linck's conclusions

[1] *Neues Jahrb.*, Beil. Bd. 16 (1903), pp. 495–513.

[2] *Jour. Washington Acad. Sci.*, III (1913), 302–4.

[3] *Bull. Geol. Soc. America*, XXV (1914), 745–80.

and to discount the importance of the algal theory. To quote from him: "The dead algal cells in the Salt Lake oölite are regarded as cells which had selected the oölite as a point of attachment. They became imprisoned within it by the further accretion of aragonite by chemical precipitation." He suggests that the decay of the attached algae furnishes Na_2CO_3 which acts as a precipitating agent and thereby aids the growth of the oölite.

As regards the importance of algae in the production of the oölites of Great Salt Lake, future studies may be expected to throw additional light on the problem. Microscopic examination of these by several investigators has failed to reveal any indications of algal structure in the calcareous grains themselves. On the other hand, they exhibit highly developed radial and concentric structure.

THE PRAIRIE DU CHIEN OÖLITE

Some time ago the writer had occasion to examine microscopically a siliceous oölite which marks the base of the Ordovician in northeastern Iowa, and found to his surprise that the oölite grains of this showed undoubted algal structures. The bed in question constitutes the so-called transition member between the Prairie du Chien dolomite and the Saint Croix sandstone. With reference to this bed Leonard, in his "Geology of Clayton County," says:

> The lower Magnesian is not marked off sharply from the underlying Saint Croix, but there is a transition from the one to the other through from fifteen to twenty feet of calcareous sandstone or siliceous oölite. The rock is composed of clear rounded grains of quartz cemented by lime carbonate. In some beds this cementing material is quite abundant, in others there is only enough to hold together the grains. The ledges vary in thickness from a few inches to two or three feet. This siliceous oölite is well exposed in an old quarry in the river bluff one and one half miles above North McGregor. The transition beds are also seen in the section at Point Ann, just below McGregor. Here there are alternating layers of sandstone and limestone and some oölite similar to that described above.[1]

A bed of similar character and thickness has been described by Calvin[2] as occurring at the same horizon in Allamakee County, which lies directly north of Clayton. The writer has examined

[1] *Iowa Geol. Survey*, XVI (1905), 239–40.

[2] *Ibid.*, IV (1894), 61.

the member at the Point Ann exposure only, and the samples here described and figured are entirely from that locality.

Microscopic examination of the rock shows it to consist of imperfectly preserved siliceous oölite grains in a dolomitic matrix. The history of the rock is briefly as follows: Subsequent to the formation of the oölite, dolomitization set in, transforming the calcareous matrix completely, and many of the calcareous oölite grains either wholly or in part, to dolomite. Alteration then ceased and silicification of the unchanged, or only partly changed, oölite grains ensued. The irregular areas of dolomite within the interiors and the frayed-out borders of many of the silicified oölite grains are in this way accounted for. The structure of grains which were completely dolomitized prior to silicification is almost entirely obliterated, and these are often only with difficulty distinguished from the matrix.

The oölite grains range from 0.1 mm. to 1.13 mm. in diameter, and when well preserved show, in addition to the concentric and radial structure, minute sinuous, enwrapping fibers very similar to the tubules which characterize the *Girvanella* type of calcareous algae. A comparison of the microphotographs of the oölite grains with that of *Girvanella problematica* Nicholson, described and figured by Rothpletz, in his memoir entitled "Ueber Algen und Hydrozoen im Silur von Gotland und Oesel,"[1] will bring out this striking similarity (Figs. 1–6).

It should be recognized that the interwoven fibers of the oölite have been partly obliterated by silicification. Doubtless these consisted of hollow tubules filled with calcite, like those shown by *Girvanella problematica* prior to silicification.

The fibers of the organism of the oölite have an average diameter of 0.015 mm. which agrees very closely with the diameter of the tubules of *Girvanella problematica*, which varies from 0.01 to 0.018 mm., according to Rothpletz.

Typically the well-preserved oölite grains consist of an inner structureless nucleus, followed by a narrow intermediate band showing radial structure, and this again by an outer band bearing

[1] *Kungl. Svenska Velenskapsakademiens Handlingar*, Band 43, No. 5 (1908), Pl. I, Fig. 1.

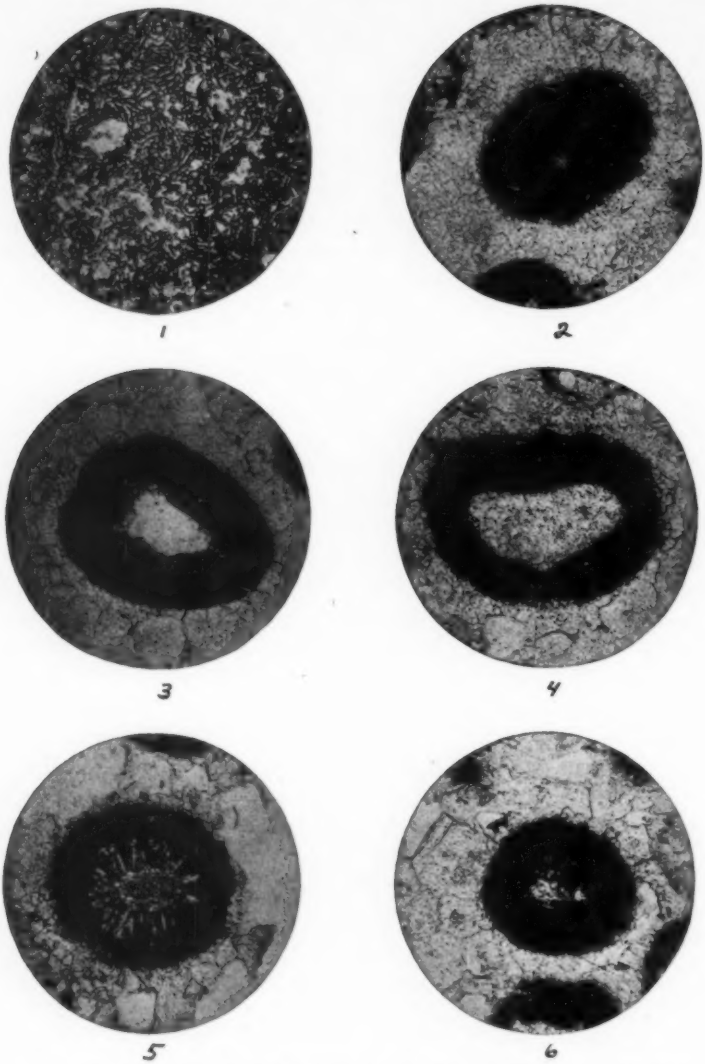

FIG. 1.—Microphotograph of *Girvanella problematica* Nicholson. About ×42. After Rothpletz.

FIG. 2.—Microphotograph of peripheral section of a silicified oölite grain from basal Ordovician at McGregor, Iowa. About ×45.

FIG. 3.—Cross-section of another grain from the same locality. About ×45. Note the well-developed algal structure in the outer portion and the band showing radial structure within this. The interior is not preserved.

FIG. 4.—Imperfectly preserved oölite grain. About ×45. The interior and peripheral portions of the grain were replaced with dolomite, with obliteration of structure, prior to silicification.

FIG. 5.—Silicified grain showing well-developed radial structure but with algal fibers nearly obliterated. About ×45.

FIG. 6.—Another grain showing fine concentric structure but with no distinct algal fibers preserved. About ×45.

sinuous fibers. In some instances, however, the two outer bands grade gradually into each other without any distinct line of demarkation; or indeed the radial structure may be entirely wanting and the concentric structure may continue into the nucleus. The fibers are best shown in peripheral sections of the grains. In these they appear to enwrap the bodies.

Some of the grains, however, show little or no trace of algal fibers, but there is convincing evidence that this fact has resulted in most, if not all cases, from the obliteration of original structures as an accompaniment of silicification. All stages of such obliteration may be traced under the microscope.

SOME EFFECTS OF CAPILLARITY ON OIL ACCUMULATION[1]

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All rocks in the upper crust of the earth contain pore space. The percentage by volume of this space varies from a fraction of 1 per cent in the case of most fresh crystalline rocks[2] up to 40 per cent in some sandstones.[3] Below ground-water level these openings are more or less saturated with water, which moves about from points of higher to points of lower pressure.

The movement of water thus entombed does not exactly follow hydrostatic laws, as can be observed by the small loss of head in artesian flow. For example, an instance is cited by Van Hise[4] where water traveled under ground 150 kilometers with a loss of only 50 m. in head. This shows that the movement was very slow (perhaps a few feet per year), for the friction through the porous stratum was almost nothing. In the case of water moving in large openings, such as pipes, friction is an important factor. A somewhat similar example was observed by the author in Missouri, where the loss of head by flow in the Roubidoux sandstone was about 200 ft. in 75 miles. A theoretical means of comparison with the observed facts is to note the size of the openings in the rocks. All tubular openings less than 0.508 mm.[5] are capillary. Therefore, by geometrical proof, it can be shown that sandstones with uniform rounded grains of less than 2 mm. in diameter, would contain mainly capillary openings. Rocks with uniform rounded grains, regardless of the size of grain, contain about the same

[1] A paper read before the Geologic Conference of Oklahoma, January 7, 1916, at Norman, Oklahoma.

[2] Van Hise, *Monograph, U.S.G.S. 47*, p. 125.

[3] G. P. Merril, *Rocks, Rock-Weathering and Soils*, p. 198.

[4] *Monograph, U.S.G.S. 47*, p. 587.

[5] Alfred Daniell, *Text Book of Physics*, p. 315.

amount of pore space, and this is greater than in rocks which have varying-sized and angular grains. Most rocks are made up of particles irregular in shape and less than 2 mm. in diameter, consequently the movement of underground water must be greatly affected by capillary action, *and evidently the forces of static capillarity must be overbalanced before movement can take place.* For that reason a discussion of Poiseuille's law of flow in capillary tubes has been omitted, and the conditions of static capillarity are thought to be of *first importance.*

The phenomenon of capillarity—that of a column of liquid rising or being depressed by a small opening—is due to two causes: (1) the surface tension of the liquid, and (2) the fact that the material of which the tube is composed has a greater or less adhesion for the liquid than the cohesion of the liquid itself.

Surface tension is the force at the surface of a liquid, which tends to make the liquid contract, and can be expressed by the following formula:

$$a) \qquad T=\frac{\pi r^2 hqg}{2\pi r \cos a},$$

where r equals the radius of the tube; h, the height of liquid standing in the tube; q, the density of the liquid; g, the acceleration of gravity; and a, the angle of contact between the liquid and the tube.

Surface tension is a linear function of the absolute temperature,[1] and that for water can be expressed by:

$$b) \qquad T=0.21(370-t)^{2}$$

where t equals the temperature Centigrade.

Pressure causes some change in surface tension, but presumably small. "For changes in the properties of water induced by pressure of, say, 1,000 atmospheres are usually similar in magnitude and direction to those observed when a relatively small quantity of a salt is dissolved in it; and the surface tension of such dilute (0.5 *N* or less) solutions differs by only a small percentage from that of pure water."[3]

[1] Knipp, *Physical Review*, XI, 151.

[2] Johnston and Adams, *Journal of Geology*, XXII, 9.

[3] *Ibid.*

Different substances have different surface tensions, which can be calculated by means of formula *a*) with the necessary observed factors. For instance, crude oil at 20° C. has an average surface tension of about 25 dynes per cm.;[1] water at 18° C. about 75 dynes; and mercury at 20° C. about 540 dynes.[2]

Surface tension also varies with the nature of materials in surfacial contact. For instance, the surface tension of mercury when in contact with water is different from when in contact with air. Unfortunately, a number of such different values are not recorded, so that this discussion is limited to liquids in contact with air.

It is necessary that the adhesion of the material in the tube be either greater or less than the cohesion of the liquid, otherwise there would be no chance for surface tension to display itself. When adhesion is less than cohesion, depression in the liquid results, as in the case of mercury and glass; when adhesion is greater than cohesion, there is a rise in the capillary tube. If adhesion greatly overbalances surface tension, the liquid surface may break and the liquid mount up the sides of the vessel, as in the case of some light oils in a low porcelain cup. Consequently, before one liquid will replace another in capillary openings the replacing liquid must not only have a greater surface tension but also a greater adhesive power for the material of which the tube is composed.

Capillary force according to equation *a*) is a function of surface tension, contact angle, diameter of pore space, density of liquid and acceleration of gravity. In the case of water-air surface the contact angle is 0, therefore (cos *a*) equals 1; the density of water is 1; so the equation resolves itself into:

$$h = kT/r,$$

where k equals 0.00204.

Starting with a temperature of 15° C., at a depth of 100 m., the capillary pressures shown on p. 801 are computed from the above formula. Pressures are recorded in kilograms per square centimeter.

The following calculations show, first, that capillary pressures decrease with depth on account of the increase in temperature;

[1] Washburn, *A.I.M.E.*, L, 831. [2] Tait, *Properties of Matter*, p. 264.

secondly, that above 750 m. capillary pressure in openings of 0.01 micron is greater than the combined rock and hydrostatic pressures; therefore capillarity is most important in the upper 3,000 ft. of the earth's crust; and thirdly, that above 5,000 ft. one liquid of greater surface tension and adhesion for the tube material should readily replace a weaker liquid in small openings; or in other words, the liquid of less surface tension should be concentrated in the larger openings.

CAPILLARY PRESSURES UNDER VARYING CONDITIONS*

Depth Meters†	Hydrostatic Pressure	Capillary Pressure for Pore Diameter of		Rock Pressure	Hydrostatic and Capillary Pressures
		100Mc	0.01Mc		
100.....	10	.03	306	27	316
500.....	50	.03	294	135	344
1,000.....	100	.027	278	270	378
2,000.....	200	.025	250	540	450

* Johnston and Adams, *op. cit.*, XXII, 13.

† An increase of temperature of 1° for every 30 m. was used to obtain these results.

Capillary phenomena can take place in openings of 0.01 micron, as shown by Bakker,[1] where he concludes that the minimum size of capillary openings is a few times the diameter of the molecule. According to Whitney,[2] mud contains more than 10,000,000,000 particles per gram. If these were perfectly round particles, so that the pore space could be a maximum, the diameter of the individual would be about 3 microns. Therefore the maximum openings would be about 0.5 micron. Clay used in the following experiments was made up of particles which varied from 1 to 5 microns in diameter, as measured by a microscope. The openings then at a maximum would be a fraction of a micron. Now, since the openings in mud are evidently less than 1 micron by both of the above methods of approach, it has been assumed for the following hypothetical problem, that in compressed shales where the particles are not round nor of equal size the openings are diminished to 0.01 micron.

[1] *Zeitschrift für physikalische Chemie*, LXXX, No. 2, 129.

[2] *U.S. Dept. Ag., Weather Bureau, Bull. 4*, p. 73.

Capillary pressure of 300 atmospheres means that water will enter the pore spaces above static water level until the pressure in the pore tubes, due to the weight of the column of liquid above or otherwise, is equivalent to 300 atmospheres pressure; or that, if the water is held back by a gas or liquid of less surface tension, it will accumulate a pressure in the said gas or liquid proportional to the difference in capillary pressures for that temperature and size of opening.

The following assumptions have been made for a hypothetical problem: (1) there exists a cavity or series of connected openings, larger than 0.5 mm., under a strip of rock 10,000 ft. wide and 1,000 ft. thick. The openings in the rock above are as small as 0.01 micron, and filled with water; (2) the material below the cavity is an oil shale in which the openings are 0.01 micron, and that water is in the lower part of this shale under sufficient head to make it rise to the level of the bottom of the cavity.

The water will drive the oil into the open cavity with a pressure equal to the difference in the capillary pressures of oil and water for that size of opening. This amount for the given temperature of 15° C. and openings of 0.01 micron is approximately 200 atmospheres, or about 400,000 lb. per sq. ft. The weight of the rock column above is approximately 150,000 lb. per sq. ft.; and that of the full water column would be less than 62,000 lb., because the column cannot possibly act upon a full square foot, but only upon the area of pore space, for convenience say 50,000 lb. Now the resultant pressure upon the rock above the cavity is 400,000 minus (150,000 plus 50,000), or 200,000 lb. per sq. ft.

This pressure acts as upon a beam fixed at both ends. The capillary water above prevents the rising of the oil into the rock, but in turn affords no downward pressure on the oil in the opening, other than the weight of the hydrostatic column, as has been accounted for in the above assumptions.

The deflection for a beam fixed at both ends with a uniform load may be expressed by the following formula:

$$d=\frac{wl^4}{384EI},$$

where d is the deflection; w, the uniform load; l, length of beam; E, the modulus of elasticity; and I, the moment of inertia.

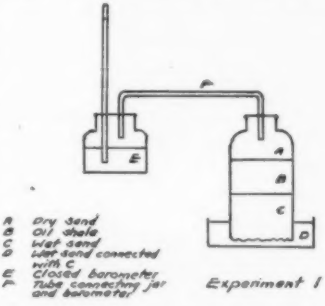
Substituting the values for a beam of rock 10,000 ft. wide, 1,000 ft. deep, 1 ft. broad, with E equal to 6,000,000 lb. per sq. in. (the value of granite), and I equal to $bh3/12$ or $(1{,}000)3/12$, the equation resolves itself into the following:

$$\frac{200{,}000\times 10{,}000\times 10{,}000\times 10{,}000\times 10{,}000\times 12}{384\times 6{,}000{,}000\times 144\times 1{,}000\times 1{,}000\times 1{,}000}$$

or approximately 72 ft. This means an anticline with a dip each way from the crest of about 1 degree.

Experiment 1

Statement.—An open glass cylinder (3 in. in diameter, and 8 in. in length) was placed in a pan of wet sand, so that the sand filled the lower one-third of the cylinder. The water had free access from the sand in the pan to the sand in the cylinder. Then a layer of oil-saturated mud was placed in the cylinder upon the wet sand; this mud occupied about one-third of the cylinder and was above the level of the water in the pan. The cylinder was then filled with dry sand, and the top sealed with a tube attachment to a closed barometer. Readings of the mercury were taken before sealing and compared with a standard barometer in the same room.

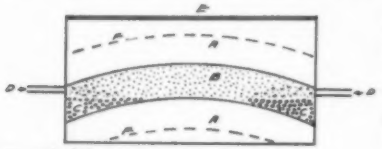


Results.—The water migrated upward about 1 cm. into the mud and the oil moved about the same amount into the dry sand. The mercury had risen within 24 hours, about $2\frac{1}{2}$ cm. over the atmospheric pressure as compared with the barometer; it then remained stationary. The oil also migrated down into the wet sand and collected in some of the larger openings.

Experiment 2

Statement.—A ($\frac{3}{4}$-in.) layer of wet sand was placed between two layers of oil in a (8 in.$\times$4 in.$\times$4 in.) rectangular glass box. The sand layer was arranged in an arched manner so that the artificial anticline dipped about 30 degrees to either side. The sand grains in the top of the curve were small (all passing a 40-mesh sieve), while those in the troughs were comparatively coarse (none passing a 10-mesh sieve). The top was sealed with paraffin and water

was allowed to enter the box through openings at the lowest horizon of the sand. This water level was never as high as the top of the curve in the sand.

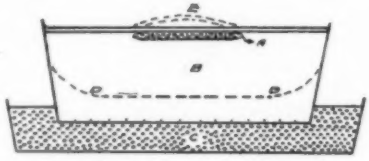


Experiment 2

Results.—The water entered the mud in both directions from the sand layer and replaced about an inch strip of the oil in the mud. The oil moved into the coarser grains of sand and within 24 hours there was an oil pool in both synclines on either side of a water-filled anticline. Later, the oil began to move out of the openings which admitted water from the outside, and collected upon the surface of the water.

Experiment 3

Statement.—A (3-in.) layer of oil mud was placed in a (round 14-in. diameter) pan, which had a number of small holes in the bottom. A circular lens of dry sand (3 in. in diameter and ½ in. thick) was fitted down in the center at the top of the mud. The surface was leveled as carefully as possible and covered with a ¼-in. layer of paraffin. This pan was then set in a pan of wet sand, so that the water level stood about 1 in. below the top of the mud in the first pan.


A Lens of dry sand
B Oil shale
C Wet sand - water having access to shale thru small holes in pan B
D-D Line of replacement of oil in shale by water
E Layer of paraffin

Experiment 3

Results.—After two weeks the paraffin had arched up over the dry sand, with a maximum rise of ⅜-in. The paraffin was punctured at this point, and within 24 hours the oil began to seep out and slowly run down the side of the dome. Several days later, seeps began to come out at various places, where the oil had dissolved its way through the paraffin. The oil also passed down out of the holes in the bottom of the pan through the sand, and collected upon the surface of the free water over the sand. Upon examination, the water had replaced about 1½ in. of the oil in the bottom of the pan.

In the foregoing experiments, the mud was made from a mixture of dried clays, the particles of which measured from 0.005 to 0.001 mm., and Oklahoma crude oil (38 Baumé). Enough oil was used to make the mud pack well.

Conclusions

At the time of this reading only the results of the elementary experiments can be given. This paper does not attempt to say that capillary forces have ever caused anticlines in nature, but merely points out that possibility. At least one thing is borne out by the above experiments: that the segregation of oil and water in openings of the ordinary oil rocks is not according to the general hydrostatic idea, but that the water forces the oil into the larger openings regardless of elevation or structure. This does not do away with the general anticlinal theory of accumulation. On the contrary, it substantiates this theory, as the larger openings are more often in the crest of the anticline, regardless as to whether the oil caused the fold or whether the oil migrated there after the fold had been made.

A PECULIAR PROCESS OF SULPHUR DEPOSITION

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The sulphur deposits of Japan have four different modes of origin: sublimation, impregnation, flow, and deposition in lakes. They are all doubtless of solfataric origin. The first two types are common everywhere around volcanic craters, but flows of sulphur are found, so far as the writer knows, only at Rausu, Hokkaido, and Tsurugisan, Rikuchu.

The lake type is very peculiar and unusual. Most of the productive sulphur mines of Japan are operated in deposits which are nearly circular in outline. Some of them which are stratified attain a thickness of 30 meters, and are overlaid by fine brown clayey or tufaceous substances which were derived partly from the surrounding rocks and partly from the sulphur itself.

The characteristic topography of the majority of these Japanese sulphur beds clearly shows that they were formed by deposition from crater lakes. The method of formation, however, appears to have been entirely different from that which produced the "gypsum" type of Sicily, Louisiana, and other places, for some were undoubtedly produced by a method similar to that which is now producing sulphur in a crater on Kunashiri, the southwestern of the Kurile Islands, nearest Hokkaido.

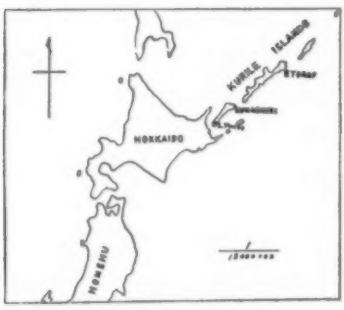

FIG. 1.—Sketch map of Hokkaido and the southern part of the Kurile Islands.

In the southwestern part of Kunashiri is Lake Ichibishinai, a crater lake 1 kilometer in diameter and 150 meters above the level of the sea. South of this lake, and 7 meters higher in elevation, there is a small circular lake

called Ponto. This small lake is 210 meters in diameter and occupies an explosion crater. When this is quiescent, the depth of the lake is from 30 to 35 meters in the center, but during periods of activity it is 30 meters deeper. Toward the margins the lake becomes abruptly shallower, as shown in Fig. 2.

The water of the lake is strongly acidic, and has a temperature of 40° C. Around the margins, through innumerable small fissures, sulphur is deposited, and the country rock is strongly impregnated with it. That the fissures extend beneath the surface of the water is clearly shown by the bubbles of gas which rise to the surface of the lake in various places. The amount of gas emitted is ordinarily not very great, but during periods of low barometric pressure enormous quantities escape. Not only do the fissures emit gas, but the conduit of the crater itself is active, and during the months of February, June, July, and August, when the barometer is low in this vicinity, periodic eruptions of hot water and gas take place.

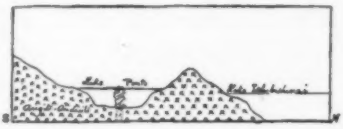

FIG. 2.—Profile of Lake Ponto and Lake Ichibishinai.

During the writer's visit to this locality in August, 1912, paroxysmal eruptions of gas and water were noticed near the center of the lake at intervals of from one to three hours, and whenever the bubbling began workmen rowed to the spot. By means of a pulley attached to a framework resting upon two boats, the men lowered a cylindrical iron bucket, with a capacity of about 140 liters, in the center of the bubbling area to the bottom of the lake. When the bucket was withdrawn, it was practically filled with sulphur grains which were formed by the union of the gases with the water in the lower part of the conduit. Being specifically heavier than water, the sulphur grains, forced upward by the ebullition, sank toward the bottom and into the bucket. In this manner, while the crater is active, a hundred buckets of sulphur are easily brought up in a day.

The greater part of the sulphur is dark gray in color, but some is yellow. The grains are hemispherical, oval, kidney-, fig-, or

spindle-shaped (Fig. 3), and they are usually about 0.2 to 3.0 mm. in diameter. The grains are not solid, but hollow, and the cell walls, which are usually rough on the outside on account of a coating of impurities and of very minute sulphur particles, are so thin that they are very easily broken. In fact, many of the larger grains are broken by mutual impact in the water. In the flat side of many of the hemispherical forms or at one end of those that are round there is a small hole, which was made by the exit of the gases which remained within it after it was formed. Most of the grains are brought up as distinct individuals, but in some cases they are united in large botyroidal masses. On account of their thin shells, they cannot keep their original forms if even slight pressure is applied, and the sunshine also destroys them.

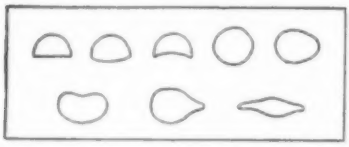

FIG. 3.—Forms of sulphur grains

Two modes of origin of the sulphur in solfataras have been suggested by geologists and chemists: first, the oxidation of hydrogen sulphide, probably according to the equation

$$2H_2S+O_2=2H_2O+2S,$$

secondly, the mutual reaction of hydrogen sulphide and sulphur dioxide, according to the equation

$$2H_2S+SO_2=3S+2H_2O.$$

The hydrogen sulphide and sulphur dioxide, emanating from the conduit, form numberless bubbles in the lake, and where they are in contact with the water the sulphur is deposited. Thus, layer after layer of sulphur may accumulate in a lake and bedded deposits be formed.

Similar oölitic sulphur grains are being formed at the present time in the crater lakes of Shirane[1] and Noboribetsu, but they are not being worked.

[1] H. Kawasaki, *Jour. Tokyo Geol. Society*, No. 122.

STUDIES FOR STUDENTS

CONTRIBUTIONS TO THE STUDY OF RIPPLE MARKS

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The two interesting papers on ripple marks by E. M. Kindle and J. A. Udden, published in the *Journal of Geology* for October-November, 1914, and February-March, 1916, respectively, suggest that readers of the *Journal* might be interested in a brief review of certain previous contributions to the subject, particularly since most of the questions raised by these two writers have occupied the attention of earlier investigators. In connection with a study of wave action, I have recently had occasion to consult a number of reports on ripple marks, and offer in the following paragraphs a synopsis of some essays not mentioned by Kindle or Udden. No attempt has been made to compile a complete bibliography of the subject, but reference is made to most of the more important papers which have come to the writer's attention.

The accumulation of sand and finer débris in parallel ridges and troughs somewhat resembling water waves in form, though not at all in origin or in method of formation, was long ago recognized as a normal product of wave and current action. Under various names, such as "current mark," "wave mark," "ripple drift," "current drift," and "friction markings," the phenomenon now generally known as ripple mark has repeatedly been described. Although not infrequently found on sandy beaches, ripple marks are perhaps better developed on tidal flats and over the broad bottoms of shallow estuaries, lakes, and ponds. They are not unknown on the deeper sea floor of the off-shore zone, where their occurrence to a depth of over 600 feet has been demonstrated. Ripple marks which are exposed by the falling tide may be

delicately dissected by rill marks, an example of this phenomenon having been described by Dodge.[1]

Among the earlier accounts of ripple marks, one of the most interesting is based on the little-known work of an ingenious French engineer named Siau.[2] In 1841 he published a brief note entitled "De l'action des vagues à de grandes profondeurs," based on observations of ripple marks in deep water, made with the aid of an ordinary sounding apparatus. While examining ripple marks, visible during quiet water, on the bed of a channel off the west coast of the Isle of Bourbon, Siau noted that the heavier particles of the sand tended to accumulate in the troughs between the ridges, while lighter material was concentrated along the ridge crests. Profiting by this discovery, he coated a sounding lead with tallow and lowered it to the sea floor where the depth was too great for direct visual observation. When brought to the surface the tallow sometimes retained, adhering to it, only heavy particles of sand, in which case the surface of the tallow had a convex form, showing that it had been pressed down into the trough between two ripples. In other cases the tallow was coated with lighter particles only, and had a concave form, as a result of having been pressed down upon a ripple crest. At great depths, where the ripples were more closely spaced, two parallel bands of materials differing in specific gravity would be impressed upon the tallow at the same time, the heavier material coating a convex ridge, and the lighter a concave depression in the tallow. By this ingenious device Siau was able to prove the existence of ripple marks at a depth of 617 feet.

The ripples described by Siau were believed by him to be due to the back-and-forth currents which are produced on a sea bottom by oscillatory waves. Such ripple marks are called "oscillation ripples," and are characterized by symmetry of crests, neither slope being steeper than the other, since the ridges are built up by currents which operate from both sides with approximately equal

[1] R. E. Dodge, "Continental Phenomena Illustrated by Ripple Marks," *Science*, XXIII (1894), 38–39.

[2] Siau "De l'action des vagues à de grandes profondeurs," *Comptes rendus de l'academie des sciences*, XII (1841), 774, and *Annales de chimie et de physique*, 3° Sér. (1841), 118.

force. The crests are sharp and narrow as compared with the more broadly rounded intervening troughs. De la Beche, in his *Geological Observer* describes another type of ripple mark, produced by the action of a current flowing steadily in one direction over a bed of sand.[1] These "current ripples" have a long, gentle slope toward the direction from which the current comes, and a shorter, steeper slope on the lee side. Sand grains removed from the gentle slope are carried to the crest and dropped down the steeper slope, causing the ripples to migrate slowly with the current, much as sand dunes migrate with the wind.

Sorby gave a very good description of current ripples in *The Geologist* for 1859, but failed to recognize the existence of wave-formed oscillation ripples, although he noted, and even pressed too closely, the analogy between true waves and ripples.[2] For many years current-formed ripples were the only type recognized in most textbooks. In 1882, in opposition to the general view, Hunt claimed that as a rule ripple marks are the product of oscillatory wave action, and supported his claim with observations based on the artificial production of ripple marks, as well as with numerous observations of naturally formed ripples.[3] He was evidently unaware of the fact that Siau had supported the same theory some forty years earlier, and in a later paper he erroneously credited Forel with priority in the recognition of oscillation ripples.[4] Hunt incidentally describes oscillation ripples in his paper "On the Action of Waves on Sea-Beaches and Sea-Bottoms."[5] He also discusses the nomenclature of ripple marks at much length in a paper published in 1904,[6] and elsewhere quotes Lieutenant Damant, R.N.,

[1] H. T. De la Beche, *The Geological Observer* (Philadelphia, 1851), p. 506.

[2] H. C. Sorby, "On the Structures Produced by the Currents Present during the Deposition of Stratified Rocks," *Geologist*, April, 1859, p. 141.

[3] A. R. Hunt, "On the Formation of Ripple-Mark," *Proc. Roy. Soc. London*, XXXIV (1882), 2, 18.

[4] A. R. Hunt, "The Descriptive Nomenclature of Ripple-Mark," *Geol. Mag.*, N.S., I (1904), 411.

[5] A. R. Hunt, "On the Action of Waves on Sea-Beaches and Sea-Bottoms," *Proc. Roy. Dublin Soc.*, N.S., IV (1884), 261–62.

[6] A. R. Hunt, "The Descriptive Nomenclature of Ripple-Mark," *Geol. Mag.* N.S., I (1904), 410–18.

as having observed ripple marks while diving at depths of 60 and 70 feet.[1]

In 1883, the year following the publication of Hunt's earliest paper cited above, there appeared three important essays on ripple marks: one by De Candolle on "Rides formées á la surface du sable déposé au fond de l'eau et autres phénomènes analogues"; another by Forel on "Les rides de fond étudiées dans le lac Léman"; and a third by Darwin "On the Formation of Ripple-Mark in Sand." De Candolle produced ripple marks artificially by experimenting, not only with sand and various substances in powdered form covered by water, but also with liquids of varying viscosity, covered with water and other liquids.[2] Regarding sand or powder mixed with water as a viscous substance, he concluded from his experiments that "when viscous material in contact with a fluid less viscous than itself is subjected to oscillatory or intermittent friction, resulting either from a movement of the covering fluid or from a movement of the viscous mass itself with respect to the covering fluid, (1) the surface of the viscous substance is ridged perpendicularly to the direction of friction, and (2) the interval between the ridges is directly proportional to the amplitude of the friction-producing movement." That ripple marks depend on simple friction alone, and not on any change of level in the covering liquid, such as occurs during wave action, De Candolle proved by an experiment with a rotating disk submerged in a tank of water. After submerging the disk and mixing an insoluble powder in the water, the apparatus was left until the powder settled on the disk and floor of the tank as an even film, and the water came to rest. An oscillatory rotary movement then applied to the disk caused radiating ripples to form upon it, while no ripples formed on the stationary bottom, and the surface of the water remained quiescent. The author concludes that the formation of ripples in sand, whether under currents of air or under water currents, is identical in origin with the formation of water ripples under moving air. If the cur-

[1] A. R. Hunt, "Facts Observed by Lieut. Damant, R.N., at the Sea-Bottom," *Geol. Mag.*, N.S., V (1908), 31–33.

[2] C. de Candolle, "Rides formées à la surface du sable déposé au fond de l'eau et autres phénomènes analogues," *Archives des sciences physiques et naturelles*, 3e Sér., IX (1883), 241–78.

rent moves always in one direction we have intermittent friction due to varying velocities. Otherwise we have oscillatory friction due to alternating change of direction. Current ripples result from the first type of friction, oscillation ripples from the second.

Forel in his excellent essay on "Les rides de fond étudiées dans le lac Léman"[1] sets forth the mature results of studies which had been briefly mentioned by him in three communications of earlier date.[2] Abandoning his first theory, that the formation of ripple marks is dependent in part upon the vertical pressure of water waves upon the bottom,[3] Forel reached the following important conclusions as the result of many careful observations and experiments: (1) Current ripples are asymmetrical and migrate with the current like ordinary sand dunes, whereas oscillation ripples are stationary and symmetrical. (2) Each oscillation ripple is really a composite of two current ripples, resulting from the action of two currents moving alternately in opposite directions, each current attempting to form the ridge into a current ripple migrating with it, but being defeated when the return current tries with equal force to shape the ridge into a current ripple directed in the opposite sense. (3) The length of the water body has no direct effect on the spacing of the ripples. (4) Other things being equal, the ripples are more closely spaced with increasing depth. (5) At a given depth, and with other conditions uniform, the ripples are more widely spaced with increase in coarseness of sand grains. (6) Ripples once formed do not experience a change in spacing as a result of diminishing amplitude of oscillation of the water, although the original spacing does depend upon the amplitude of oscillation, as pointed out by De Candolle. (7) For any given coarseness of sand grains there is a certain mean velocity of the oscillating currents which will produce ripples; lower velocities

[1] F. A. Forel, "Les rides de fond étudiées dans le lac Léman," *Archives des sciences physiques et naturelles*, 3e Sér, X (1883), 39–72.

[2] F. A. Forel, "La formation des rides du Léman," *Bulletin de la Société Vaudoise des sciences naturelles*, X (1870), 518; "Les rides de fond," *ibid.*, XV (1878), P.V. 66–68; "Les rides de fond dans le golfe de Morgues," *ibid.*, 76–77.

[3] F. A. Forel, "La formation des rides du Léman," *Bulletin de la Société Vaudois des sciences naturelles*, X (1870), 518; "Les rides de fond étudiées dans le lac Léman," *Archives des sciences physiques et naturelles*, 3e Sér., X (1883), 40.

will fail to move the sand grains, and hence cannot build ripples, while higher velocities agitate the whole mass of sand so violently that no ripples can form. (8) The formation of ripples is initiated by some obstacle or inequality on the surface of the sand, behind which sand grains accumulate in the eddy caused by its presence; this leaves a furrow on either side of the initial ridge, due to the abstraction of sand accumulated in the ridge; and these furrows in their turn cause additional ridges to develop on their outer margins, and so on. (9) In a given locality, ripple marks almost always form with the same spacing, regardless of the varying intensity of winds and waves affecting the water body; this is in consequence of laws 7 and 6 stated above. (10) The depth at which ripple marks may form is limited by the depth to which wave action may extend with sufficient energy to move the bottom sands; hence it depends on the size of the waves, and therefore in part indirectly on the size of the water body; in the Rhone, the limiting depth is a few decimeters; in Lake Geneva, some ten meters; and in the ocean, from 20 to 188 meters, according to Lyell and Siau. Forel revised De Candolle's law regarding the relation of ripple spacing to the amplitude of the friction-producing movement to read: "The breadth of the ripples, or the distance from one crest to another, is the length of the path followed during a single oscillation by a grain of sand freely transported by the water." The length of this path varies directly as the horizontal amplitude of the oscillatory movement of the water, directly as the velocity of that movement, inversely as the density of the sand, and inversely as the size of the sand grains.

Darwin's paper "On the Formation of Ripple-Mark in Sand" is especially noteworthy for its careful analysis of the vortices which are so important a factor in the construction of the ripples.[1] When symmetrical oscillation ripples were subjected to the action of a steady current, Darwin noticed that not only did sand grains migrate up the weather slope of each ripple with the current, but that they also ascended the lee slopes, apparently *against* the current. This proved conclusively the existence of vortices. Darwin

[1] G. H. Darwin, "On the Formation of Ripple-Mark in Sand," *Proc. Roy. Soc. London*, XXXVI (1883), 18–43.

then proceeded to study the vortices by watching the movements of a drop of ink released from the end of a fine glass tube at that point in the water where the action was to be observed. In this manner the vortices associated with the alternating currents which produce oscillation ripples were analyzed with a high degree of precision, and much light was thrown upon the method of ripple growth. Darwin concluded that "the formation of irregular ripple marks or dunes [current ripples] by a current is due to the vortex which exists on the lee side of any superficial inequality of the bottom; the direct current carries the sand up the weather slope and the vortex up the lee slope. Thus any existing inequalities are increased, and the surface of sand becomes mottled over with irregular dunes." The intermittent friction which De Candolle adduced is not essential in this explanation of current ripples. Oscillation ripples of regular pattern are changed by a continuous current into regularly spaced current ripples; but a uniform current cannot of itself initiate regularly spaced ripple marks. "Regular ripple mark [oscillation ripples] is formed by water which oscillates relatively to the bottom. A pair of vortices, or in some cases four vortices, are established in the water; each set of vortices corresponds to a single ripple crest." Forel's conception of an oscillation ripple as a composite of two current ripples formed alternately by oscillating currents is regarded as correct; but his law for the relation of ripple spacing to amplitude of oscillation is believed to require some modification.

Further studies of ripple-forming vortices were made by Mrs. Hertha Ayrton, the results of which were not published until 1910.[1] With the aid of well-soaked grains of ground black pepper, or of particles of potassium permanganate dissolving and coloring the water while the latter was in oscillation, she observed the formation of vortices and endeavored to explain the mechanics of their growth. Although she expressed disagreement with the conclusions of Darwin and others on certain points, most of her results afford essential confirmation of their main contentions. Some doubt must attach to certain of her deductions, such as

[1] H. Ayrton, "The Origin and Growth of Ripple-mark," *Proc. Roy. Soc. London*, Ser. A., LXXXIV (1910), 285–310.

one to the effect that no ripple-forming vortex occurs in the lee of an obstacle over which a steady current is passing, and that hence "a steady current is unable either to generate or to maintain ripple mark."

The British Association Reports for the years 1889, 1890, and 1891 contain three papers by Reynolds on the action of waves and currents in model estuaries, in which are some valuable observations regarding what may well be termed giant tidal ripples.[1] While experimenting with artificial tidal currents, Reynolds discovered that current ripples were formed in the model estuaries. By making due allowance for the difference in size between the model estuaries and those in nature, he concluded that real tidal currents ought to produce very large current ripples, possibly 7 or 8 feet in height and 80 to 100 feet apart.[2] Some years later Vaughan Cornish discovered natural tidal ripples of the same type as those produced artificially by Reynolds, having a height of 2 feet and an average distance of more than 37 feet from crest to crest.[3] In two later papers Cornish described giant tidal ripples more fully, and illustrated their essential features with a large series of beautiful photographs.[4] Some of these ripples have a height of nearly 3 feet above the intervening troughs, and a distance between crests of from 66 to 88 feet in extreme cases. The giant ripples are often covered with ordinary ripple mark, and while Cornish recognized that the larger forms were produced by the continuous steady flow of tidal currents, he was at first inclined to invoke pulsatory currents in order to explain the smaller ripple mark.[5]

[1] Osborne Reynolds, "Report of the Committee Appointed to Investigate the Action of Waves and Currents on the Beds and Foreshores of Estuaries by Means of Working Models," *Rept. British Assoc.* (1889), pp. 327–43; *ibid.* (1890), pp. 512–34; *ibid.* (1891), pp. 386–404.

[2] *Ibid.* (1889), p. 343.

[3] Vaughan Cornish, "On Tidal Sand Ripples above Low-Water Mark," *Rept. British Assoc.* (1900), pp. 733–34.

[4] Vaughan Cornish, "Sand Waves in Tidal Currents," *Geogr. Jour.*, XVIII (1901), 170–202; "On the Formation of Wave Surfaces in Sand," *Scottish Geogr. Mag.*, XVII (1901), 1–11.

[5] Vaughan Cornish, "On Tidal Sand-Ripples above Low-water Mark," *Rept. British Assoc.* (1900), p. 733; "Sand Waves in Tidal Currents," *Geogr. Jour.*, XVIII (1901), 197–98; "On the Formation of Wave Surfaces in Sand," *Scottish Geogr. Mag.*, XVII (1901), 8.

This theory seems to be a survival of De Candolle's erroneous idea that "intermittent friction" is essential to the production of current ripples, and is practically abandoned by Cornish in his more recently published book on *Waves of Sand and Snow*.[1] Gilmore described tidal ripples on the Goodwin Sands having a height of "two or three feet."[2]

It should be noted that all of the giant ripples referred to above belong to the asymmetrical type; they are true current ripples. So far as I am aware no giant oscillation ripples have ever been observed along modern shores. It may be doubted whether tidal currents could form symmetrical ripples, notwithstanding Reynold's suggestion to the contrary.[3] The flow and ebb of the tide constitute an oscillating current, it is true; but the currents are often of unequal force. Where equally strong, each current persists long enough to remodel the ridges formed by the preceding current, giving them an asymmetrical form appropriate to the current operating last. On the other hand, Gilbert has described structures in the Medina sandstone formation of New York which he believed to be giant ripples of the symmetrical type, formed by oscillating currents due to wave action.[4] In dimensions these ridges were similar to the average examples of tidal ripples described by Cornish, having a height of from 6 inches to 3 feet, and a distance from crest to crest of from 10 to 30 feet; but their nearly symmetrical form did not suggest a similar origin. Gilbert reached the tentative conclusion that they were formed by waves 60 feet high in deep water of a broad ocean. This conclusion was criticized by Fairchild, who advanced convincing arguments in support of the opinion that the forms in question were beach structures, possibly successive beach ridges built on the strand.[5] Branner

[1] Vaughan Cornish, *Waves of Sand and Snow* (London, 1914), pp. 289–90.

[2] John Gilmore, *Storm Warriors, or Lifeboat Work on the Goodwin Sands* (London, 1874), pp. 108–9.

[3] Osborne Reynolds, "Report of the Committee Appointed to Investigate the Action of Waves and Currents on the Beds and Foreshores of Estuaries by Means of Working Models," *Rept. British Assoc.* (1889), p. 343.

[4] G. K. Gilbert, "Ripple-Marks and Cross-Bedding," *Bull. Geol. Soc. Amer.*, X (1899), 135–40.

[5] H. L. Fairchild, "Beach Structure in the Medina Sandstone," *Amer. Geologist*, XXVIII (1901), 9–14.

suggested that they might represent fossil beach cusps seen in cross-section.[1]

In 1911 A. P. Brown published a paper entitled "The Formation of Ripple-Marks, Tracks, and Trails," in which he endeavored to show that asymmetrical ripples (current ripples) are formed by deposition, whereas symmetrical ripples (oscillation ripples) result from the erosion of a formerly smooth botton, consequent upon the rippling of overlying water by wind action.[2] His conclusions do not appear to be supported by a sufficient body of convincing evidence, and are opposed by theoretical considerations and by the great body of experimental data already referred to. In presenting his theory this author makes no reference to the many previous investigations of ripple marks of all kinds, the important results of which have been summarized above.

Ripple marks have repeatedly been discussed in connection with the interpretation of fossil ripples found in sedimentary rocks. We need mention but a few of these discussions in the present connection. As early as 1831 Scrope described fossil ripple marks found on slabs of marble, and explained them as due to the oscillatory movements of shallow water.[3] Darwin, starting from the very questionable assumption that a great ebb and flow of the tide is essential to the formation of numerous ripples, concluded that the presence of a large number of ripple marks in a geological formation indicates with a considerable degree of probability that the tides of early times rose higher than those of today.[4] Van Hise figured and described one type of oscillation ripples, and emphasized their value as criteria for determining the original altitude of steeply inclined strata.[5]

[1] J. C. Branner, editorial note, *Jour. Geol.*, IX (1901), 535–36.

[2] A. P. Brown, "The Formation of Ripple-Marks, Tracks, and Trails," *Proc. Assoc. Nat. Sci. Philadelphia*, LXIII (1911), 536–47.

[3] G. P. Scrope, "On the Rippled Markings of Many of the Forest Marble Beds North of Bath, and the Foot-Tracks of Certain Animals Occurring in Great Abundance on Their Surfaces," *Proc. Geol. Soc. London*, I (1831), 317–18.

[4] G. H. Darwin, "On the Geological Importance of the Tides," *Nature*, XXV (1882), 214.

[5] C. R. Van Hise, "Principles of North American Pre-Cambrian Geology," *Sixteenth Ann. Rept. U. S. G. S.*, Part I (1896), 719–21.

Spurr showed that where continuous deposition takes place from a current which constantly maintains asymmetrical ripples on the surface over which it flows, the forward movement of the ripples combines with the deposition of heavier and larger fragments in the troughs and lighter particles on the crests to give a peculiar type of false bedding in the resulting formation.[1] Jaggar criticized Spurr's conclusions on the ground that his own experiments and observations indicated that ripple marks could not be produced in heterogeneous material;[2] but Spurr met the criticism with a fuller discussion of the matter in which his original contention is well sustained.[3] A short time previously Sorby had described a somewhat similar phenomenon in a paper[4] printed almost exactly half a century after the publication of his first account of ripple marks, already cited. From an examination of the "ripple-drift" type of false bedding in rocks, Sorby believed that one could "ascertain with approximate accuracy, not only the direction of the current and its velocity in feet per second, but also the rate of deposition in fractions of an inch per minute."[5] Additional discussions of fossil ripple marks are cited by Kindle in his paper referred to at the beginning of this article, but need not be repeated here.

[1] J. E. Spurr, "False Bedding in Stratified Drift Deposits," *Amer. Geologist*, XIII (1894), 43–47.

[2] T. A. Jaggar, Jr., "Some Conditions of Ripple-Mark," *Amer. Geologist*, XIII (1894), 199–201.

[3] J. E. Spurr, "Oscillation and Single-Current Ripple Marks," *Amer. Geologist*, XIII (1894), 201–6.

[4] H. C. Sorby, "On the Application of Quantitative Methods to the Study of the Structure and History of Rocks," *Quart. Jour. Geol. Soc. London*, LXIV (1908), 180–85.

[5] *Ibid.*, pp. 181, 197–99.

Reviews

Geology of Saratoga Springs and Vicinity. By H. P. Cushing and R. Ruedemann. New York State Museum; Bull. No. 169, 1914. Pp. 177, pls. 20, figs. 17, maps 2.

Scientific interest regarding Saratoga Springs and vicinity centers about its mineral waters, and this report has been published in response to a demand for detailed information on local geological conditions.

Rocks of Pre-Cambrian, Cambrian, and Ordovician age outcrop in the area. The Paleozoic rocks are divided into deposits of eastern and western troughs, characterized by different sets of formations. The western trough was being eroded in Lower Cambrian times, but in the east the Georgian is the only Cambrian present. The rocks of the western division are horizontal or nearly so, but in the east the beds are intensely folded and crumpled. Two great normal faults with a number of branches cross the Saratoga quadrangle. These are known to be genetically connected with many of the mineral springs.

A unique feature is the Northumberland volcanic plug. It outcrops just north of Schuylerville as a knob of extrusive rock and is unlike any other igneous rock in the state. It has been connected with one theory for the origin of the mineral springs, but unfortunately the authors were unable to determine with certainty whether the rock is in place or not, and are in doubt in regard to calling it a volcanic neck or a fragment of a surface flow.

It was planned to have Professor Kemp write a chapter for this bulletin on the origin of the mineral waters but his results were published in an earlier report. The authors are not convinced that Kemp's conclusions are justified by the field evidence. Kemp holds that the mineral waters, in part at least, are of magmatic origin. He cites as proofs their local occurrence, the volcanic neck, the large amount of free CO_2, and the almost complete absence of sulphates. The authors believe that the absence of carbonated waters to the north is due to lack of shale covering and resulting dilution with surface waters. They hold that the volcanic knob furnishes no evidence of igneous activity of sufficient recency to justify connecting it with present-day juvenile waters. The abundant CO_2 may come from deeply buried impure limestones and

shales. The absence of the SO_4 radicle does not dismiss the possibility of connate waters as a source of the mineral salts. The sulphates originally in the connate waters may have been lost as the waters moved along toward the surface by some such chemical reaction as the precipitation of gypsum by the action of sodium sulphate on calcium carbonate in the presence of free CO_2.

Thus the chief problem that this quadrangle offers is held in question still, but this is not due to lack of skill or painstaking effort on the part of the authors of this report. It is a worthy contribution to the geological literature of this state. W. B. W.

Genesis of Pyrite Ores of St. Lawrence County. By C. H. SMITH, JR. New York State Museum, Bull. No. 158, 1912, pp. 143–82. Figs. 29.

Under the most favorable conditions, definite conclusions regarding the geneses of ore bodies cannot always be drawn, and when these are found in bodies of rock as highly metamorphosed as the Grenville series many complications arise.

In this area, pyrite is widely disseminated, but the ore bodies are associated only with "rusty gneisses thought to be metamorphosed impure sandstones and shales." The writer believes that the metallizing period was subsequent largely to the main period of metamorphism, and was brought about by magmatic emanations permeating the gneisses and replacing with pyrite certain minerals which are usually very stable. These emenations came from the abundant intrusions after active movement of the magmas had ceased. It is not stated that the pyrite all came from the magmas. In fact, to explain the association of the ore bodies with the gneisses alone, it is suggested that only the sulphur was of igneous origin, and that the iron was furnished by the metamorphosed sediments. To cover minor occurrences of pyrite three additional periods of formation are postulated, but are not considered to have been of importance in determining the ore bodies.

Additional points of interest are found in the lack of association of the ore bodies with gabbros, as some authors have stated in other areas, and possible genetic relations of pyrite with associated graphite.

The explanation of the ore bodies strikes one as quite involved, but the author assures us that it is in very small proportion to the complexity of field problems and conditions.

W. B. W.

Geological History of New York State. By WILLIAM J. MILLER. New York State Museum, Bull. No. 168, 1914. Pp. 130, pls. 52, figs. 40.

This bulletin is a brief summary of the geological history of the state. It was the intention of the author to presuppose no scientific knowledge of geology on the part of his readers, and that the work should be in the nature of a textbook. A few pages in the introduction are devoted to geologic processes and throughout the context an effort is made to define technical terms. The reviewer does not believe the author succeeded in making the report sufficiently non-technical to be popular with laymen. It will serve better as a reference book for geologists who wish a brief statement of some of the larger phases of the region's history.

The report is illustrated with many excellent photographs of unconformities and other structural and physiographic features which abound in the state.

Unfortunately this report, in common with other New York reports, does not contain a table of contents and its value as a reference book is impaired thereby.

W. B. W.

Origin of Hard Rock Phosphates of Florida. By E. H. SELLARDS. Florida Geol. Survey, Fifth Annual Report, 1913, pp. 23–80, pls. 9, map 1.

The hard rock phosphates are found chiefly as bowlders and irregular fragments in a formation of Pliocene age that the author has named Dunnellson. The formation is rather heterogeneous but a phase of light-gray sands is the usual matrix in which the phosphate rocks are imbedded.

Theories generally advanced to explain these deposits have involved some form of guano alteration. The author believes the real source of the phosphate was from phosphoric acid derived from the disintegration, *in situ*, of overlying beds. The acid was borne downward by ground-water, and replaced limestone, or was chemically precipitated. No reactions are suggested for the latter process. The deposits are associated with clay lenses and other conditions that interfere with the free circulations of ground-waters. It is suggested that the presence of precipitating agents may be the important factor here rather than the retardation of ground-water circulation. The shattered and hetero-

geneous character of the formation is explained by the caving in of solution cavities and their subsequent refilling.

The theory presented seems to explain the larger features of the phosphate deposits, but the report should be considered a statement of progress of investigation, rather than the last word in explanation of the deposit.

W. B. W.

Water Supply of Eastern and Southern Florida. By E. H. SELLARDS. Florida Geol. Survey, Fifth Annual Report, 1913, pp. 113–288, pls. 5, figs. 17, map 1.

This report covers in detail an area of twenty-two counties in which, for the most part, the artesian waters may be tapped by flowing wells. This area includes the outer rim of counties along the eastern, southern, and southwestern borders of the state.

The principal aquifer is the Vicksburg limestone of Oligocene age. Underlying the whole state, this formation is exposed in the central part and dips beneath younger formations to the east and south. These younger beds have not been well differentiated and some wells may obtain water from them, but strong flows are from the Vicksburg. The structure includes a low anticline with its axis dipping gently to the east in the central part of the state. The water-bearing horizon is 100 feet below the surface along the coast, and near the crest of the anticline. In the northeast corner of the state the wells are from 300–400 feet deep and at the southern extremity from 900–1,000.

The gentle dip of the strata does not furnish strong pressure in any locality and a head of 25 feet is rather exceptional. Local topography affects the distribution of the flowing wells.

In some areas there has been great development of the artesian water supply. There are not less than 500 flowing wells in the city of Jacksonville. Statistics covering recent years show a progressive loss of flow from the wells in this city.

Much of the artesian water of the state is not potable on account of mineral salts, chiefly sodium chloride. This is notably true in the southern part. All the underground water of the state is very generally charged with hydrogen sulphide, but its use for domestic purposes is not prevented thereby.

A small area of flowing wells in the western part of the state is not treated in detail in this report.

W. B. W.

Geology of North Creek Quadrangle. By WILLIAM J. MILLER. New York State Museum, Bull. No. 170, 1914. Pp. 90, pls. 14, figs. 9, map 1.

This quadrangle lies wholly in Warren County, New York, in the southeastern Adirondacks. It is of geologic interest chiefly because of certain rock types and structures. At the present time no rocks of later age than Pre-Cambrian are present, but Paleozoic outliers just off the map seem to prove that late Cambrian and probably early Ordovician sediments have been removed. The Grenville series makes up the meta-sedimentary rocks, and the author believes the evidence favors their Archeozoic rather than Proterozoic age. This series has a limestone member of remarkable thickness, 10,000–12,000 feet, and below this is 3,000 feet of quite pure quartzite.

About 60 gabbro outcrops are shown on the map, usually with elliptical ground-plans. Their form is that of small stocks or bosses, rather than dikes. The author believes these gabbro occurrences furnish strong evidence in favor of Daly's magmatic stopping and assimilation hypothesis. The igneous masses were not intruded by pushing aside the country rock, but rather by a process of replacement. Marked primary variations in the gabbros and the presence of inclusions as xenoliths are cited in support of this theory and seem to make a strong case.

Garnets are present in the area in quantities of some economic importance. Some of the occurrences are attributed to the assimilation of Grenville sediments, and subsequent crystallization from "original magmas." This use of the term "original magma" for a magma that has assimilated considerable quantities of sediments is questionable.

W. B. W.

The Waverlian Formations of East Central Kentucky. By W. C. MORSE and A. F. FOERSTE. Kentucky Geol. Survey, Bull. No. 16. Pp. 76, figs. 5.

Stratigraphic relations of the Mississippian beds in Kentucky are of interest for economic reasons. In Ohio and West Virginia formations in the Waverly series are oil- and gas-producers and their extension into Kentucky is a fact of considerable importance.

This report covers twelve counties in the east-central part of the state. The beds were traced southward from known sections in Ohio,

making the correlations fairly certain. The sections show that toward the south the sandstones of the oil horizons of Ohio rapidly grade into shales. Even the shales of the Bedford and Berea formations become very thin although they do not disappear. These changes are unfavorable for oil-bearing sands in the southern part of the state.

A number of changes in correlations in formations of the Waverlian are made from those given in earlier Kentucky reports and in the Richmond Folio. The latter part of the bulletin treats of the possibilities of these beds in producing building stones and clays. If a map of the area covered by the report had been given it would have made part of the discussion more intelligible.

W. B. W.

The Geology of the Rolla Quadrangle. By WALLACE LEE. Missouri Bureau of Geology and Mines, XII. Pp. 117, pls. 12, figs. 17, maps 2.

The area covered by this report is in the central Ozark region of Missouri and includes Phelps and Dent counties. The strata described include the Gasconade, Roubidoux, and Jefferson City formations. The general horizon is of interest because it includes part of the Ozarkian and the Canadian of Dr. Ubrich's classification. The author follows the usual classification, placing these beds in the Upper Cambrian. A few erosion remnants of Carboniferous age are found in the northeastern part of the area.

An interesting structural feature is found in a number of sink areas. The author believes it was developed from the caving and subsequent filling of solution cavities.

The economic products of this quadrangle are negligible and the chief value of the report lies in its contribution to the general stratigraphy of the region.

W. B. W.

Glass Sands of Oklahoma. By FRANK BUTTRAM. Oklahoma Geol. Survey, Bull. No. 10, 1913. Pp. 91, pls. 8, figs. 3.

Approximately one-half of this report is taken up with a general description of the glass industry. As the author is a chemist he has treated chemical processes in glass production rather fully.

Notable glass sand deposits of the state are limited to three areas: the Arbuckle Mountains, southeastern Oklahoma, and near Tahlequah,

in the northeastern part of the state. The greatest deposits are in the Arbuckle Mountains, in the Simpson formation of Ordovician age. This formation is from 1,200 to 2,000 feet thick, and the sands outcrop at four horizons. Five sections give an average thickness for the glass sands of 248 feet. The supply of raw materials seems almost inexhaustible but transportation facilities are lacking in most localities. In southeastern Oklahoma the Trinity sandstone at the base of the Cretaceous carries commercial quantities of good glass sand, and several localities are readily accessible. In the northeast the Burgen sandstone, which has been correlated with the St. Peters, carries a 50-foot bed of high-grade glass sand, but is too remote from railroads for present development.

Analyses of these sands show that they compare very favorably with deposits now being worked in adjacent states. Having a marked advantage in the use of natural-gas fuel, Oklahoma sands should prove strong competitors for the glass market of the central Mississippi Valley.

W. B. W.

Inland Lakes of Wisconsin. By EDWARD BIRGE and CHANCY JUDAY. Wisconsin Geol. Survey, Bull. No. 27, 1914. Pp. 132, figs. 8, tables 4, maps 29.

A large portion of the data in this report has been published in various bulletins scattered through a dozen years. It seemed desirable to gather this material in a single volume, together with additional data not published hitherto.

No lakes occur in the southwest or driftless area, and all the lakes of the remaining three-quarters of the state are of glacial origin. In general the lake basins were formed in four different ways: by melting of blocks of ice imbedded in the glacial débris, by damming of preglacial valleys, by interlocking of terminal moraine deposits, and by inequalities in deposition of ground moraine.

The total number of lakes runs into the thousands, but only the larger ones are described. There are 21 hydrographic maps, and with each is a brief report on the geology and topography of adjacent regions and the origin of the lake basins.

Tables give data on the locations of the lakes, their size, and the depth and shape of their basins. Lake Winnebago with an area of 215 square miles is by far the largest in the state. Few of the lakes exceed one hundred feet in depth. The United States Geological Survey has

estimated the total lake area of the state at 810 square miles. The authors believe that twice this amount is more nearly correct.

W. B. W.

Preliminary Report on Tertiary Paleontology of Western Washington. By CHARLES E. WEAVER. Washington Geol. Survey, Bull. No. 15, 1912. Pp. 80, pls. 15.

A Tertiary invertebrate marine fauna of 246 species is listed in this report. Eighty-four of these are new species and are described and figured for the first time. The fauna is very largely pelecypods and gastropods.

Lower Eocene rocks are absent. The Upper Eocene fauna totals 79 species. The Oligocene fauna is limited to 10 species. A detailed report will supplement this bulletin later and treat more fully of the stratigraphic and structural relations.

W. B. W.

Geology of East Central Oklahoma. By L. C. SNIDER. Okla. Geol. Survey, Bull. No. 17, 1914. Pp. 25, pls. 2, fig. 1.

The area treated in this report includes all of Haskell County and portions of five adjoining counties. It deals with structural features almost entirely and the stratigraphy given follows United States Geological Survey reports.

About twenty anticline and syncline axes are plotted. Well-drillers may locate the axes of anticlines roughly from this map and supplement it by detailed work in each locality. For the convenience of many who have not access to the annual reports of the United States Geological Survey, the report includes a map and descriptions of the principal folds in a region adjacent on the southwest. A number of wells are producing gas in these two areas, but oil wells of importance have not been reported.

W. B. W.

Ponca City Oil and Gas Field. By D. W. OHERN and R. E. GARRETT. Okla. Geol. Survey, Bull. No. 16, 1912. Pp. 30, pls. 2, fig. 1.

The Ponca oil and gas field is located in north-central Oklahoma near the Kansas line. It produced gas only until 1911 when the first oil

well was brought in. Thirty producing oil wells were operating at the time this bulletin was written.

The report describes the formations of Lower Permian and Pennsylvanian age that outcrop in the Ponca City area, and also those underlying that outcrop to the east and west. The structure of the Ponca City anticline is shown by a contour map on the surface of the Herington limestone.

It is the opinion of the authors that many of the wells labeled "dry" are not deep enough to test their localities. Some holes do not go down 1,000 feet, and few below 1,600; but the approximate position of the lowest oil sand is much deeper, and the anticline will not be tested thoroughly until wells have reached the Tucker sands at a depth of nearly 3,500 feet.

W. B. W.

The Mineral Springs of Saratoga. By JAMES F. KEMP. New York State Education Department, Bull. No. 517, 1912. Pp. 79, figs. 8, tables 7.

There are few problems more difficult for geologists than those connected with the origin of mineral springs. The district centering at Saratoga Springs has long been famous for its mineral waters, and this report has been prepared in response to the very general interest regarding them. The report takes up briefly a historical sketch of the springs, the local geology, and a general description and classification of groundwaters.

The chemical composition of the water is known by analyses of three different periods, 1838, 1871, and 1905. These show a total of ten acid and twelve basic ions. The most abundant salt is sodium chloride followed by calcium, magnesium, and sodium bicarbonates. The waters carry an average of two or three volumes of CO_2 in solution. The sulphate ion is practically absent.

The author rejects any theory that attributes the springs to connate waters, the absence of sulphates being the strongest chemical evidence against such theories. The same geological section is faulted in many other places in the Hudson and Champlain valleys, yet even uncarbonated brine springs are lacking elsewhere. The author's conclusion is that many of the mineral constituents, as the haloids, sodium carbonate, and the carbonic acid gas, are from deep-seated sources. The tendency of dying volcanoes to give off abundant CO_2 and the occurrence

within ten miles of the only purely volcanic rock in New York, Vermont, or western Massachusetts support this theory. The carbonated waters take on calcium and magnesium carbonates from the Little Falls dolomite on their upward journey. This conclusion accords with the marked tendency of economic geologists in the last decade to lay greater stress on the importance of magmatic emissions.

W. B. W.

Coal Resources of District No. I (Longwall). By GILBERT H. CADY. Illinois Coal Mining Investigations, Bulletin No. 10, Urbana, 1915. Pp. 149, pls. 9, figs. 27, tables 24.

The Longwall District, comprising Bureau, Putnam, Marshall, La Salle, and Grundy counties and the adjacent parts of Livingston, Kankakee, and Will counties, an area of about 1,700 square miles, contains nearly six billion tons of available coal and is one of the foremost districts of the state in economic importance. This bulletin is concerned with the stratigraphic and structural geology of the region, the economic geology of the coals and accompanying strata, and with the working data developed. The important beds are Nos. 2, 5, 6, and 7, of which No. 2 has been extensively mined. These coals have been studied in a large number of mines. The character of the coal beds and their general structure have been worked out in detail, and many sections through the productive coal measures have been tabulated. In addition to its value in connection with the coal resources, the bulletin is of general interest in that it contains an outline of the geology of the La Salle anticline, including Starved Rock, Deer Park, and the surrounding country.

A. D. B.

Coal Resources of District No. VII. By FRED H. KAY. Illinois Coal Mining Investigations, Bulletin No. 11, Urbana, 1915. Pp. 233, pls. 4, figs. 47.

District No. VII comprises Macoupin, Madison, St. Clair, Christian, Montgomery, Bond, Clinton, Washington, Perry, Moultrie, Shelby, Fayette, Marion, and parts of Sangamon, Macon, and Randolph counties, an area of about 7,000 square miles, containing coal estimated at more than forty-five billion tons in bed No. 6 alone. The stratigraphy of the coal measures has been carefully studied, and numerous sections have been measured and tabulated. Some interesting structures in the coal

beds have been noted, and illustrated by a number of diagrams. The rocks of the area are confined to the Pennsylvanian, with comparatively simple structure. No. 6 is the only coal bed producing important quantities of coal.

A. D. B.

Notes on Geology of the Gulf of St. Lawrence. By J. M. CLARK. New York State Museum, Bull. No. 158, pp. 111–20.

The author treats briefly of the geology of Entry Island. Of chief interest is his description of a type of topography which he calls "demoiselle." The relief is due to numerous mammiform hills rounded into softly contoured domes of striking symmetry. These domes are caused by the erosion of small laccoliths.

W. B. W.

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